MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1870, *TO JUNE* 1871.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXI. *November* 11, 1870. No. 1.

William Lassell, Esq., President, in the Chair.

G. F. Rodwell, Esq., 14 Denbigh Place, Belgrave Road,
Francis Barrow, Esq., 3 Phillimore Gardens,
Howard Grubb, Esq., Dublin,
C. Bird, Esq., Bideford; and
John Sidebotham, Esq., George Street, Manchester,

were balloted for and duly elected Fellows of the Society.

The Index to the first twenty-nine volumes of the *Monthly Notices* is now ready for delivery to the Fellows in the same manner as the *Memoirs*, *i.e.* by application to the Assistant-Secretary at the rooms of the Society, either personally or by an authorised agent. The price to persons not Fellows is 5*s*. It is to be obtained of the Society's publishers, Messrs. Williams and Norgate, Henrietta Street, Covent Garden.

Note from the Astronomer Royal.

Perhaps I may be permitted to make use of the *Monthly Notices* as a channel for conveying, to our scientific correspondents of foreign countries, a notice of the danger to the packets transmitted by post, arising from the flimsiness of the paper in which they are enveloped. Some time since, an empty Italian envelope reached me; after some inquiry, the pamphlet which it had inclosed was found. Subsequently, an empty American envelope came; its contents have not been found, and their subject is not known to me. Lately, an envelope, indorsed as from an Italian meteorological observatory, and tied up in our Post Office, was found to contain a Belgian newspaper; probably some ardent politician has been disappointed on finding in his envelope a set of meteorological observations.

October 20th, 1870.

Note on the Zodiacal Light. By Richard A. Proctor, B.A.

It cannot but be regarded as a remarkable circumstance that the nature of the zodiacal light should in the present state of Astronomy continue to be a *quæstio vexata.* I do not here refer to the physical constitution of this object, respecting which we may possibly be unable for many years to form a satisfactory theory, but to the determination of the actual position of the zodiacal light in space. Astronomers have been able to determine from geometrical considerations the paths of such objects as comets and meteors; it would therefore seem that the position of such an object as the zodiacal light ought ere this to have been determined.

Yet it must be admitted that there are peculiar difficulties in this problem. We can reason respecting the distance and motion of a comet, because we know that our observations are made on one and the same body whose motions are in accordance with the laws of gravity. It is otherwise with respect to the zodiacal light. We see a certain glow or radiance occupying a definite position with respect to the horizon and to the celestial circles; but we have no means of ascertaining whether the objects from which that radiance proceeds are the same at any one time as at any other, or indeed (as will presently appear) whether a single one of the constituents forming the zodiacal gleam at one season is present within the same region of the solar system at another.

The geometrical considerations applicable to the zodiacal light are, however, too definite to admit of question,—in other words, the path to be followed in seeking for a theory of this object is unmistakable. Hitherto, so far as I am aware, that path has not been traced out *far enough* for the attainment of definite views,—the perplexities which presently surround us as we follow it having seemed perhaps to render further research hopeless.

It happens, however, not unfrequently, that the very difficulties surrounding a subject of this sort assist us,—in this way—that they enable us to reject theories which otherwise might engage our attention and so cause perplexity. Precisely as the very complexity of a lock makes us all the more certain that a key which opens the lock is the key really appertaining to it; so where a subject of astronomical research presents many perplexing phenomena these become so many reasons the more for accepting a theory which is not contradicted by any one of them.

This is, I think, the case with the zodiacal light. We are able—as I hope now to show—by considering the peculiarities of this object, to get rid, one after another, of various theories which might otherwise distract our attention. And though by this process of elimination we may not be enabled to determine quite the true theory of this object, we can yet considerably narrow the field within which selection has to be made.

The first considerations to be dealt with are those which depend on the normal features of the zodiacal light. It is well known that the light exhibits usually the figure of an oblique conoid whose axis lies close by the ecliptic, and whose vertex lies at a varying distance from the position of the Sun. Near the axis the light grows brighter, except close by the vertex, where it is even fainter than at the other parts of the border. The following table, prepared by Herr Klein, from modern observations, indicates the varying range of the vertex from the place of the Sun,—though it must be remembered (and will be recognised at once by every one familiar with the varying position of the ecliptic during the year, and other like circumstances) that these measures indicate variation in the extent of visibility rather than (of necessity) any real variation in the extent of the light.

Day of the Year.		Distance of Vertex from Sun.	Part Observed.
January	2	83°·0	Western half.
	26	91·8	,,
February	11	81·0	,,
March	14	74·0	,,
April	14	75·0	
May	4	65·0	,,
August	1	77·0	,,
September	15	58·0	Eastern do.
October	17	74·5	,,
November	12	71·3	,,
	29	56·5	,,
December	13	61·0	,,
	28	80·5	,,

The setting of the zodiacal light when the western half is visible, and the rising of the light when the eastern half is visible, take place quite regularly, and in a manner precisely corresponding with what would be observed if the zodiacal light were a distant object like a planet, a star, or a portion of the the Milky Way.

Now these circumstances at once enable us to reject the theory that the zodiacal light is a terrestrial appendage — by which I understand for the moment an object lying within the Earth's atmosphere. For there can be no question whatever that if any definite portion of our atmosphere were rendered luminous in any way, that portion would either occupy an unchanged position, or would shift according to the laws regulating the process of illumination, or according to the winds, or other like terrestrial causes. Now that on any given occasion such causes might so operate as to give the illuminated air the appearance of rising or setting as celestial objects do (that is, not *merely* rising or setting, but rising

or setting along declination parallels) is quite possible, however unlikely. To take an illustrative instance: a balloon, seen at any one instant between an observer and the Sun, might be carried by the winds so as to continue between him and the Sun, even until the hour of sunset. But to suppose that night after night at any station a relation so peculiar would characterise the illuminated air, is like supposing that a balloon, started day after day from a given place, would day after day fulfil the condition considered above. This is obviously incredible. But even if it were credible, it would be insufficient, since the region of our atmosphere which would have to be illuminated in order to account for the zodiacal light as seen in one place, would, as seen from other stations, present an appearance wholly different from that of the zodiacal light. In fact, if the former place were in England, the zodiacal light would actually be in the zenith of places 900 miles or so west or east of England.

Next we have the normal aspect of the zodiacal light in different latitudes to consider. Now we have the most positive assurances from astronomers of eminence that the zodiacal light, wherever seen, occupies ordinarily precisely those regions of the heavens corresponding to the theory that it is too far from the Earth to have an appreciable parallactic displacement. We have the evidence of practised astronomers like the Astronomer Royal for Scotland, Captain Jacob, and others; and all the evidence we have points to the conclusion that the zodiacal light, as seen in the tropics, extends at any moment over those same parts of the stellar heavens which it illuminates as seen from our northern stand-point. The *limits* of the light may seem greater in those latitudes than in ours, but the axis of the conoidal gleam is situated precisely as with us.

Now it seems wholly unquestionable that this quality of the light should dispose at once and for ever of the theory that the zodiacal light is due to the existence of a ring of matter around the Earth.

Let it be remembered that there is only one way in which the ordinary aspect of the zodiacal light can at all be interpreted on such an hypothesis. If there were a ring of meteorites as fa from us as the Moon is, then undoubtedly there would be gleam in the west after sunset, and in the east before sunrise, i the position where we see the zodiacal light. And further, th individual meteorites producing any portion of that gleam woul undoubtedly rise and set much as the zodiacal light is observe to do. But there would also be a gleam, and a much bright gleam, in the south. The meteorites rising and setting wou turn only a small portion of their illuminated faces towards u those in the south (on or close by the ecliptic) would be "full so to speak, and their combined lustre would be proportionate more considerable. Now supposing the ring exactly coincide with the ecliptic, the Earth's shadow would fall on the p due south. But the width of this shadow would (on the sup

sition we are considering) be relatively small. At midnight, in our latitudes, we should undoubtedly, on this supposition, see two arms of light extending from the eastern and western horizon along the ecliptic, each growing brighter and brighter towards the south; and a relatively narrow black rift would lie between the bright extremities of these arms. It is no theory that this would be the case, but a simple deduction from the most obvious geometrical laws.

If, then, we are to have a ring round the Earth, it must lie far within the Moon's orbit, so that the Earth's shadow may be wide enough to cover the meteorites along the whole of that long arc which under ordinary circumstances is undoubtedly unilluminated. The Earth's shadow cannot be more than 8000 miles across *anywhere*, and we must have our ring at such a distance that this width of 8000 miles may correspond to (or subtend) that wide arc of darkness actually observed under ordinary circumstances. (It is absolutely essential that ordinary circumstances should be accounted for; only when this has been done need we begin to inquire into extraordinary circumstances.)

Now we need not leave our own latitudes to decide how far off the ring should be to account for the apparent dimensions of the zodiacal light; because on the theory that the Earth's shadow, falling on a ring of some sort, defines the limits of visibility of the light, it would follow, precisely as in the case just considered, that the light would grow brighter and brighter up to the very edge of the shadow. (Supposing that edge to correspond to the extent of the Earth's shadow, there would be a somewhat ruddy bordering; but up to the commencement of that fringe there would be a regular increase of brilliancy.) But passing over this consideration (and also the consideration that the observed aspect of the zodiacal light in our latitudes is wholly inconsistent with the aspect thus shown to be due to the hypothesis we are dealing with), we may take as most favourable to the hypothesis of a meteoric ring near the Earth those observations of the zodiacal light in tropical regions which give to the ordinary apparitions of the light the greatest observed extension from the Sun.

We have it on the authority of Professor Piazzi Smyth that, even when he observed the zodiacal light under exceptionally favourable conditions, from an elevation, namely, of no less than 11,000 feet above the sea-level, the western tongue had completely set fully four hours before the eastern tongue began to rise. Now even if the eastern tongue were just beginning to rise when the western tongue had fully set, there would still be an arc of 180° between the two vertices.* But the shadow of the Earth would not account for such an arc as this between the vertices, unless the outer part of the ring had a radius not exceeding $\sqrt{2} \times$ radius of the Earth (even in the most favourable

* It must be remembered that each vertex, as the zodiacal light was seen by Professor Smyth, lay close by the ecliptic.

case of a station near to the equator), and with such a radius as this the outer part (even) of the ring would be always invisible from places having a higher northerly or southerly latitude than 45°.

And even if we set this demonstration on one side for a moment, it is yet obvious that whether a ring lying relatively near the Earth coincided in plane with the equator, or with the ecliptic, or with any intermediate plane, it could not possibly exhibit any approach to coincidence with the celestial ecliptic, when viewed from high latitudes. Further, as seen from high northern latitudes, such a ring would always have a parallactic displacement causing it to lie to the south of its geocentric position, and *vice versâ:* whereas no such association between the latitude of the observer and the apparent position of the zodiacal light has ever been observed ; far less such a systematic association as the case requires.

It is geometrically impossible, then, that the ordinary aspect of the zodiacal light can be accounted for by any theory which represents it as due to a ring of light-reflecting bodies around the Earth, whether that ring be close by the Earth or at a distance comparable with the Moon's.*

We need not consider the theory that the light may be due to a self-luminous ring around the Earth, for obvious reasons.

Now, passing from the normal features of the ring to more or less exceptional peculiarities, we find ourselves compelled to reject yet one other theory of the light—I mean the theory that it is due to a disk of minute bodies travelling in orbits of small eccentricity around the Sun.

The peculiarities which oppose themselves most strikingly to this theory are those which relate to the position and extent of the zodiacal light, though it will be obvious that the observed variations in the apparent brightness of the light are not readily explicable on this hypothesis.

Admitting the existence of a disk of bodies, travelling as sup posed, it will be evident that the changes affecting the motions o any member of the system would correspond exactly to thos

* While dealing with the relations presented by the Saturnian ring-system in 1864, I was led to apply the formula, with suitable changes of elements, t the case of a ring circling the Earth; being invited to the inquiry by the perus of the observations made by Lieut. Jones, and comments made thereon b Baron Humboldt. I found that there is not a single hypothesis as to tl dimensions of such a ring which would lead to results according with or even the slightest degree approaching the results of observations made upon tl zodiacal light. This conclusion is embodied in a note at p. 117 of that treatis in which note I remark that such investigations "prove that the zodiacal lig cannot be due to a ring of a minute satellite surrounding the Earth, the appea ance of the ring in high latitudes being altogether different from that whi would be presented by a ring surrounding the Earth." I am careful to refer these researches and their results, because remarks have been published impl ing that I have somewhat hastily come to a decision on the points here de with. A complete mathematical investigation of the subject, made fully years since, may be regarded as fairly meeting those remarks.

which would affect the motions of any considerable orb travelling at a similar distance from the Sun. In other words, the changes would resemble those slow periodic changes which affect the orbits of the Earth, *Venus*, and *Mercury*. Nor is it conceivable that the members of the system would so interfere with each other's motions as to affect appreciably at any time the appearance of the disk. Now changes such as these, affecting the individuals of a set of bodies which at any one time were spread with a certain uniformity (as the ordinary appearance of the zodiacal light would imply to be the case with its constituents) could not account for the observed changes in the position and extent of the light. The axis of the gleam has been seen at times, by practised observers, inclined at a considerable angle to the plane of the ecliptic. The extent of the zodiacal light has varied at times in the most remarkable manner, while its luminosity has been so variable that sometimes for months together it has been scarcely perceptible (in our northern latitudes); while at others it has been singularly conspicuous. I set on one side for the moment those observations by Lieut. Jones which would imply that at times the zodiacal light increases so greatly in extent as to become visible at once both on the eastern and western horizon. I also set on one side those observations by M. Liais according to which the zodiacal light can be seen at times extending as a complete arch from the eastern to the western horizon. Assuming these observations to be reliable (and those by M. Liais do not seem open to question), a true theory of the zodiacal light may be expected to account for them. But without insisting on this, it is evident, I think, that the admitted variations of the zodiacal light, in position, extent, and splendour, do not admit of being interpreted by the theory that the light is due to a disk, including always the same materials moving in orbits of small eccentricity.

Nor do our difficulties seem removed if we assume that the constituents of the disk travel in orbits of considerable eccentricity, so long as we suppose that the actual constitution of the disk is constant, or nearly so, amidst whatever variations in the distribution of individual constituents.

Yet the general aspect of the zodiacal light, and the considerations already applied to other theories, suffice to prove that there is always present around the Sun as centre a disk, whether composed of discrete meteorites, of vaporous masses, or of some combination of these and other forms of matter. The materials of this disk must be in motion around the Sun in accordance with the laws of gravity; at least we have no evidence whatever inviting us to the supposition that they differ in this respect from all the other constituents of the solar system.

We are thus led to the conclusion that the bodies composing the zodiacal light travel on orbits of considerable eccentricity, carrying them far beyond the limits of what we may now term the zodiacal disc. The constitution of the disc thus becomes

variable, and that within limits which may be exceedingly wide. They must be so in fact, if all the recorded variations of the zodiacal light are to be accounted for. In other words, it is requisite (if our evidence is to be explained) that the paths of the materials comprising the zodiacal light shall be not only for the most part very eccentric, but that along those paths the materials should not be strewn in such a way that a given portion of any path is at all times occupied by a constant or nearly constant quantity of matter.

According to this view the constituents of the zodiacal light would—at least as respects distribution along their several paths and the general figure of those paths—resemble very closely the meteoric systems which, as we know, the Earth traverses in the course of her annual motion around the Sun.

By considering the zodiacal light we have thus been led to a theory involving, and associated with, the theory of meteor-systems as now established by the labours of Adams, Leverrier, Schiaparelli, and others. But it is worth noticing that by reversing the process, and considering first the theory of meteor-systems so established, we are led quite as readily to the theory that there must at all times exist in the Sun's neighbourhood a disk of discrete constituents which would present precisely such an appearance as the zodiacal light. I have shown elsewhere that this result is a simple mathematical deduction from the evidence.

But setting this consideration wholly on one side, the fact remains that all other theories of the zodiacal light—that is, of the motions of its constituent parts, without reference to its physical constitution—have been eliminated. It remains only to be shown that this theory is controverted by no peculiarities in the observed appearance of the zodiacal light, and also that we should inquire what further general laws, if any, may be predicated of the motions of the bodies composing this object.

The fact that the axis of the zodiacal light is ordinarily close to the ecliptic, is accounted for on the assumption that the various paths along which the constituents of the zodiacal disc travel, tend to aggregate towards the neighbourhood of the ecliptic. There is nothing, however, to prevent individual systems from having a considerable inclination to that plane.

The observed variation of the zodiacal light in brilliancy, position, and extent, is obviously to be expected according to the view of its structure now under consideration.

The simultaneous appearance of an eastern and western light and Liais's observation of a complete arch of light, have to be accounted for as highly exceptional, but at the same time recognised phenomena. It is easy to see that both these phenomena may be regarded as indicating the occasional but very exceptional extension of the zodiacal disk to a considerable distance beyond the orbit of the Earth. But it must not be concealed that there are grave difficulties to be removed before this interpretation can be regarded as satisfactory.

Let us suppose, for instance, the case of a thin luminous disk occupying the whole orbit of *Mars*, and that the Earth is in the part of her orbit where her distance north or south of this plane is greatest. Then it will be evident that the outline of the disk as seen from any part of the Earth would correspond very nearly to a great circle of the heavens, and that the whole of the visible heavens south or north of that great circle would be hidden by the luminous disk. In other words, a region of the heavens far larger than that occupied by the arch of Liais, or by the eastern and western lights of Jones, should be occupied by the zodiacal light if it had some such extension as we have assumed in the case of this luminous disk.

It is to be remembered, however, that assuming (as we are bound to do) a considerable degree of flatness in the actual figure of the zodiacal disk, and more especially of its more distant portions, then much more light would be received from those parts towards which the line of sight is directed at a considerably acute angle, than from those parts which the line of sight crosses nearly at right angles. And it is easy to see that on any reasonable assumption as to the range of zodiacal substance which it is necessary that the line of sight should traverse in order that any appreciable light should be received, the occasional visibility of the light where the superior planets alone can be seen becomes as readily explicable as the ordinary visibility of the light in those parts of the sky where the inferior planets become visible.

It will be seen that all that can be strictly said to have been demonstrated in this paper is the fact that the zodiacal light is associated with the Sun, and not with the Earth ; that it is not due to solar light reflected from bodies travelling within the Earth's orbit, whether in circular or elliptic orbits, and that if the major part of the zodiacal light is reflected solar light, then the paths of the bodies reflecting that light must resemble those of the meteors encountered by the Earth. As the spectroscope seems to show that at least a portion of the light* of the zodiacal gleam is not reflected solar light, we cannot, in the present state of our knowledge, definitely decide on a theory as to the motions of the bodies to which the light is due. For the solution of the problem is obviously bound up with the interpretation of the physical nature of the zodiacal light. If some solar action, for example, rouses luminosity in certain definite directions—as, for instance, near the plane of the Sun's equator—in some such way as light is caused to appear along radial lines through and

* I use this mode of speaking not by any means as doubting the accuracy of Angström's observation ; but because even if the greater part of the light gave a continuous spectrum, yet this spectrum might remain undiscernible even when bright lines corresponding to a very minute proportion of the total light were seen with ease. Nay, such bright lines as Angström found in the spectrum of the phosphorescent light from the sky might be detected when a continuous spectrum from the much brighter light of the zodiacal radiance remained unseen.

beyond the heads of comets, our power of theorising from such considerations as have been dealt with in this paper would be limited. It would still remain certain that the zodiacal light is not a terrestrial appendage (either near or far off), but what sort of solar appendage it might be would be a problem as difficult to solve as that presented by comets.

If the radiated structure of the Sun's corona as seen under favourable atmospheric conditions should be confirmed as more than an optical phenomenon, it is not impossible that we might be put in the way of interpreting the zodiacal light.

On the Performance of a Zenith Sector constructed for the Great Trigonometrical Survey of India by Messrs. Troughton and Simms, from the Designs of Lieut.-Col. A. Strange, as reported by Capt. J. Herschel, Royal Engineers. By Lieut.-Col. A. Strange, F.R.S.

I have from time to time informed the Society of the progress in construction of an extensive series of large instruments designed by me for the use of the Great Trigonometrical Survey of India, — comprising a Great Theodolite with 3-foot horizontal circle, two 5-foot Transit instruments with Clocks and Chronographs for determination of longitude, two Zenith Sectors with 4-foot telescopes for determination of latitude, and various other minor apparatus.

Of these, one of the Zenith Sectors (No. 2) has reached its destination and has undergone the test of a season's work in Southern India in the hands of Capt. J. Herschel, Royal Engineers, some extracts from whose report on its performance may be of interest to the Society. First, however, I will submit a brief description of the instrument.

Students of astronomical history are familiar with the zenith sectors employed by the elder geodesists. Ramsden's Sector, minutely described in Pearson's *Astronomy*, may be taken as a type of these. It consisted of a telescope nearly 8 feet long, suspended by the object-glass end from the top of a wooden framing 12 feet high, the eye end, armed with a micrometer tangent screw, traversing a short fixed graduated arc or sector. This instrument displays great ingenuity, particularly in the illuminating appliances. Even at the present day it is well deserving of study. It was, however, exceedingly cumbersome in transport, and, as I have often heard the late Sir George Everest say, a most troublesome instrument to adjust and use. That distinguished surveyor substituted for it powerful Alt-azimuths, two of which were employed in India from his time until recently for latitude observations. In about 1836 a Zenith Sector was designed by the present Astronomer Royal for the Ordnance Survey, in which work it has been extensively employed. The "Airy Zenith Sector" is fully

described, with illustrations, in the volumes of the *Ordnance Survey*. Its form is in keeping with modern ideas of astronomical instruments, and it constituted an immense advance on all previous sectors.

Some years ago Sir Andrew Waugh, the late Surveyor-General of India, and afterwards Colonel J. Walker, his successor, the present head of the India Survey, determined to have the latitude observed at far closer intervals in the triangulation than had heretofore been thought necessary; their object being to elucidate the obscurity surrounding the phenomenon of local attraction, an obscurity which still tends to affect with doubt all geodesical elements. Both these officers were of opinion that the sector principle offered advantages for taking these observations, and two Zenith Sectors were accordingly sanctioned by the Government of India, who called upon me to design them.

The form of the Airy Sector, or some modification of it, would naturally have been adopted, had it not been expressly pointed out by Colonel Walker that the weight of that instrument (1140½ lbs. without the packing-cases) rendered its transport in mountainous districts devoid of roads an absolute impossibility. It was, however, necessary that I should avail myself of such ideas as might be suggested by an instrument designed by so eminent a master, and employed with such excellent effect in the the English Survey. Sir Henry James was good enough to place it entirely at my disposal, permitting me to take it to pieces and to observe with it at Southampton under the auspices of Serjeant-Major Steel, who had worked with it extensively. The conclusions to which this study of the subject led me were—First, that I must entirely abandon the principle of Mr. Airy's design; but that, secondly, his method of observing was so perfect, that any future sector must be suitable to the employment of that method.

The Airy Sector revolves azimuthally between two fixed external points. These points are supported by two lofty cast-iron pillars, the weight of which is a large portion of the entire weight of the instrument. It was evident that a self-contained vertical axis would require far less ponderous masses. This was, therefore, the basis of my plan.

The arrangement ultimately adopted by me was, in general terms, as follows:—A wide based cast-iron column about two feet high contains within it a strong conical steel vertical axis whose lower extremity, of hemispherical form, works in a cup adjustable laterally for the purpose of levelling the instrument. The upper end of the vertical axis (omitting details) carries a cradle containing two bearings for the support of the horizontal axis. The cradle also supports four micrometer microscopes for reading off the sector divisions. The horizontal axis carries at one end the sectors 3 feet in diameter, and the telescope of 4 feet focus, at the other a counterpoise. When the telescope is directed to the zenith, a diameter passing through the centres of the two sectors is horizontal, whereas in Airy's Sector this line is vertical.

The four sector micrometers are arranged conically and the divided face of the sectors is bevelled, by which means a single light, fixed about 5 feet from the centre of the instrument, illuminates all four micrometers, very much in the same way as that employed in the Greenwich transit-circle. The instrument has no horizontal circle whatever, but has two stops which admit of its being reversed promptly in azimuth as is the case with the Airy Sector, so that stars can be observed twice at the same culmination, in order to eliminate collimation and other instrumental errors.

Some conception of the instrument may be given by saying that it bears a general resemblance to an Equatoreal of the so-called German form set up as it would be for use at the pole, the vertical axis of the sector corresponding to the polar axis of the Equatoreal, and the transit axis to the declination axis.

It would be impossible to render the details of the instrument and its numerous adjustments intelligible without many illustrations, for entering on which extensive work my present duties leave me no time. I hope at some future period to execute this task in the fullest manner.

It should be mentioned that the method of determining latitude known by the several names of the "differential," the "zenith telescope," and the "American"* method, having of late years attracted much notice, I felt it right to supply my Zenith Sector with the appliances required for this method, in order that the two rival systems,—the differential and the absolute,—might be tested with the same identical telescope and mounting.

Before ordering the work to be commenced I submitted my designs for the instrument to the Astronomer Royal, who was good enough to examine them, down to the minutest details, with great care and attention. The general approval which he accorded to them gave me a confidence in their soundness which I could not otherwise have felt. He, however, considered that the gain in lightness claimed for my instrument, which I had assumed from mere inspection of drawings of his sector and mine to the same scale, should be strictly ascertained. I, therefore, computed the weight of each separate part from my drawings, a task which occupied me incessantly for a fortnight. I found the total weight by computation to be 600 lbs., against 1140½ lbs., the weight of the Airy Sector. So far the problem of designing a much lighter instrument with equal optical and measuring power appeared to be solved, the questions of convenience in use and efficiency of performance remaining to be tested. It is a curious, and in some degree I believe an accidental fact, that the instrument when completed and actually weighed was found to be 595½ lbs., that is, within 4½ lbs. of the computed weight.

The instrument, after undergoing very strict examination and

* This I believe is an incorrect appellation. The method is indicated in Pearson's *Astronomy*, and is said to have been first used by English observers.

actual use in determining latitude at Lambeth Observatory, and after receiving many alterations and additions, was at length despatched to Madras in October 1869 by the overland route, under the special charge of Lieut. Rogers, R.E., an officer of the Survey, who had thoroughly mastered its principles under my instructions. It arrived at Bangalore in November 1869 in perfect safety, and was immediately employed in field operations by Capt. J. Herschel of the Survey Department.

I was naturally most anxious to hear how the instrument had performed, for I well knew how many parts of it admitted of doubt from their novelty, and how many others I now saw ways, as I believed, of improving. Capt. Herschel, with a consideration I shall never forget, did not suffer me to remain long in suspense. Immediately on the conclusion of his season's work, whilst still engaged in field duties, he wrote me a letter, commenced "Coimbatore Base, 20th March, 1870," and concluded "Bangalore, 4th April," from which I will now make a few extracts. He says,—

"I am now observing with your Sector at my sixth station, since receiving it in the end of November last. In the interval I have taken about a thousand zenith distances, or perhaps twelve hundred. I cannot spare the time to count them up now. I am, therefore, in a position to speak without hesitation as to the facility and comfort with which the work can be performed. . . .

"Of course you will prefer a perfectly honest judgment even if the approval is not altogether unqualified. But you need not be afraid; for, in truth, I shall find it difficult to find fault. The ease with which the observations can be taken is proved by the fact that I now make five minutes my limit in choosing the Star-list — not that that is the *smallest* interval allowable, but the interval at which I can, if called upon, take three or four stars in succession. It could be done quicker, but at some sacrifice of precision. You know exactly the work that has to be done in that time, so I need say nothing to bring out the force of the evidence. There can be no better proof," Capt. Herschel adds, "of forethought and efficiency of design and execution in an instrument, than that it allows of rapidity, and certainty, and comfort in manipulation, and spares the observer mental and physical distractions and distress."

The facility of manipulation here spoken of by Capt. Herschel will be rendered intelligible by the statement that in the brief interval of five minutes no less than twenty-four distinct operations, consisting of settings, readings, intersections, and reversals, have to be performed. The testimony to this feature in the instrument is peculiarly gratifying to me, as I have always held that convenience in use is an absolute essential in an instrument intended for taking long series of observations, and that the designer should spare no pains to render the tedious labour of the observer as easy as possible. According to my experience this maxim does not always receive due attention. But gratifying as

the commendation is to myself, I should have shrunk from citing it if it did not reflect at least equal credit on the makers of the instrument, Messrs. Troughton and Simms, and on the skill of Capt. Herschel in using it.

As to permanence of adjustment, Capt. Herschel writes:—

"I am using the microscopes *as I found them*; that is to say, I have re-adjusted nothing as to run and focal distance; simply because there was no occasion to do so. This proves," he adds, "that these adjustments are of a nature to stand a good deal of travelling."

With regard to the sectors, Capt. Herschel says:—

"The graduation seems to be almost perfect. I question," he remarks, "whether the error of position of any division exceeds $\pm 2''$, as compared with the mean of all."

On the subject of instrumental and observational error the following passage occurs:—

"My observation record is made to show the *zero error of microscopes* at every observation—generally before passing on—so that mistakes may be detected on the spot. This is a joint test of graduation, intersection, and observation errors, as well as of stability; and it is satisfactory to find that the extreme range is not greater than $\pm 1''$ for six or seven hours' work."

The consistency of the instrument with itself, or its power of repeating, is thus reported on:—

"I take fifty stars at each station, three times each. I find that the zenith distances, corrected for dislevelment, agree with each other, so that all the members of each triplet, *in about three cases out of four*, fall within a range of $1''$."

The accuracy attained in the final results is defined as follows:—

"At the last station twenty-two stars out of the list were *Nautical Almanac* stars, so that the latitude was easily deducible. Cutting out *Procyon*, which has a large proper motion, and, I suspect, an erroneous place; and γ' *Virginis*, which has almost certainly an erroneous place, due to confusion of the two components, the remaining twenty give latitudes all falling within a range of $1''\cdot6$."

"I may be too sanguine," Capt. Herschel goes on to say, "but I think I see here a glimpse of something *beyond local latitudes*. It is impossible, in the field, to go far in computations without risk, and I would not be over-confident, but the *order of accuracy* indicated by these flying reductions has no parallel in my experience,—a not very extensive one I know. I take no credit," Capt. Herschel adds, "on the score of observation. I attribute it wholly to the instrument, and believe it can do still better. As regards latitude only," he states, "I am satisfied that one night's observation (of thirty-six stars in six hours) will suffice to give a result whose probable error will be *not greater than one-fifth of a second*. [In the case I have mentioned," he adds, "it is $\pm 0''\cdot 11$, but this is from twenty stars taken three times

each."] "If I am right in this, and the event will prove it in two or three months, your Zenith Sector will have to be promoted to the task of determining *places*, as well as latitudes."

Capt. Herschel points out various omissions and imperfections regarding which I entirely agree with him. He says the instrument will not give first-rate results as a Transit instrument, in which he is quite right. It is unsymmetrical, and has not, to use the apt expression of Sir John Herschel, to whom I sent his son's letter, sufficient "hold on the meridian." It will, however, determine time, when clamped in a manner I have since pointed out to Capt. Herschel, with an accuracy ample for observatory purposes, but certainly not for determining Right Ascensions.

I had particularly requested Capt. Herschel to test the differential method of latitudes, and on that point he writes:—

"I have said nothing about differential zenith distances; but the method has not been ignored. I have sufficient observations of that kind to give a pretty good idea (when they are worked out) of the relative advantages. My impression is that it is a waste of power, with such an instrument."

This opinion, guarded though it is, merits some weight as coming from Capt. Herschel, who, in a former letter written before he had used the instrument, made the following remark on this very subject: "I anticipate that the advantages of the N. and S. star transits (in pairs) will, in the long run, put zenith sector readings out of court."

I may, perhaps, be permitted to say a few words on this subject myself. The differential method has undoubtedly the great advantage of requiring a much less weighty, less complicated, and less costly instrument than the absolute method; it also practically eliminates refraction doubts; and the reductions are far more simple. But, on the other hand, the absolute method admits of a greater number of results being obtained in a given time,—a matter of no small importance in extensive geodesical undertakings in wild or unhealthy countries; and it also admits of a given amplitude being determined by the simultaneous observation, at the two extremities of an arc, of the same identical stars with two similar instruments worked by two sets of observers, on the very same nights. This plan was most successfully carried out by the late Sir George Everest on the Great Meridional Arc of India, and it has the invaluable merit of being entirely independent of the places of the stars observed. As to the relative accuracy attainable by the two rival methods, that time and very careful extensive series of observations alone can decide.

So far as my experience or knowledge extends, this is the first instrument of importance sent to India which was found fit and ready for immediate use on its arrival. This is to be ascribed to no particular skill on any one's part, but to the principle on which the system of inspection in my office is based,—namely, that no instrument shall be considered as complete until the very observations for which it is intended shall have been

taken with it. Seven years' experience has proved to me that no examination, however detailed and laborious, is equivalent to the above precaution.

I think it desirable to draw particular attention to this matter with reference to the extensive series of instruments now being constructed for observing the transit of *Venus*. Unless these are actually set up and used precisely as they will be for the observation of that phenomenon, some disappointment is almost certain to arise, whatever the skill and forethought of the most eminent makers may have done to secure success.

Aurora Borealis of 24 *Sept.*, 1870. By Rear-Adm. Ommanney.

A brilliant display of the Aurora Borealis was visible here last night; I submit the following statement of my observations, which probably may be acceptable to the Society.

About 8.30 P.M. a broad white streak of light was seen due north extending from the horizon towards the North Star; this streak was soon accompanied by several others of considerable brilliancy, rising and streaming upwards from the horizon between the N.W. and N.E. points, and converging towards the zenith, at the same time all the northern portion of the sky became illuminated.

At 9 P.M. the coruscations of the Aurora became more brilliant and active, streaming with great rapidity towards the zenith, but none could be seen to reach that point, assuming very beautiful colours, which were generally of light pink and reddish hues.

About 10 P.M. the colouring and brilliancy of the Aurora attained their fullest effect, the coruscations varying with yellow, pink, and almost crimson colours, the intermediate spaces of the sky in the background appearing to be light green, the brightest streamers were confined to the north or north-westerly directions; the coruscations continued in rapid action with surprising grandeur up to 10.30 P.M. when a somewhat indistinct purple arch was visible, with its culminating point north, and about 35° above the horizon; shortly after this exhibition, the display of coruscations and luminous streamers gradually ceased action, but the northern portion of the sky remained illuminated, resembling twilight after sunset.

The sky was quite clear at the commencement of the Aurora, it became partially obscured about 10 P.M., the clouds, especially those to the N.E., reflected very bright roseate hues from the Aurora, which in combination with the varying colour of the coruscations produced a most striking and beautiful spectacle. Three shooting stars were observed between 9 and 10 P.M. and several were seen on the previous night.

The barometer read 30.40, and has not been below 30.30 during the past week; the temperature has been very regular for

the week, the maximum by day 64°, and the minimum at night 51°; winds have been constant from the eastward, the sky free from cloud.

The northern region of the sky remained illuminated throughout the night, producing a strong reflection on the sea, and rendering the colouring of the rocks and vegetation around me quite perceptible, even at 3.30 A.M. the light from the northward produced the effect of early dawn.

I must add that during all my Arctic voyaging I never witnessed in any Aurora the same conditions of varied colouring as were displayed on this occasion.

It may be of interest also to state, that on the 21st inst. I observed a bright meteor to shoot in a path from near the Polar Star to *Capella*, vanishing near the latter after discharging a brilliant light green flash.

From Ilfracombe the view to the north commands an extensive range of sea horizon.

Ilfracombe, Devon, 25th Sept. 1870.

On the Spectrum of the Aurora Borealis.
By John Browning, Esq.

During the display of the Aurora Borealis which occurred on the evenings of the 24th and 25th of October, I confined my attention to observing the spectra of the light, taking it in different parts of the sky. When the spectroscope was directed to the more luminous portions, which were generally of a silvery white, the spectrum appeared to me to consist of only one line. I could not succeed in verifying the position of this line, but it appeared to be situated between D and E in the spectrum. When observing the light of the red portions of the sky, a faint red line became visible. I had no means of verifying the position of these lines with any degree of exactitude, but I was able to throw into the field of view a faint continuous spectrum from a distant light, and also the bright yellow sodium line produced by a spirit-lamp.

The colour of the green line was very peculiar; had I not been able to observe it by comparison I could not have formed any idea of its position. It was an exceedingly light silvery green, or greenish grey, and often seemed to flicker. Besides the two lines I have particularly described, I occasionally suspected others, one in the red, and one in the blue, but I could not be at all sure of this. The colour of the light of the Aurora seen over the greater portion of the heavens resembled exactly that of the discharge of electricity from an induction coil through a vacuum formed from atmospheric air.

Further Observations on the Currents of the Solar Photosphere: Lateral, Ascensional, and Cyclonic. By C. H. Weston, B.A. Cant., F.R.A.S., F.G.S.

The remarkable extent of disturbance which the solar photosphere has lately exhibited must have arrested the attention of most telescopic observers. Towards the end of August there was a train of groups and spots (more or less remote) extending nearly across the upper part of the Sun's disk, over a surface of about 400,000 miles. Since this period the trains of spots, although contracted both in extent and numbers, have yet been very many, and have afforded fine objects for critical examination. But as it is only by minute and exclusive attention to one particular spot, and endeavouring when practicable (*de die in diem*) to connect in *sequence* its successive changes, both in form and direction, that we can hope to acquire any increased insight into solar physics. I selected, after a general survey, one large elliptical spot for continued study. It was in northern latitude and surrounded by groups of minor openings and ramified faculæ, betokening alike the actual presence of energetic activity and the promise of future development, and destined most fortunately to exhibit subsequently the interesting and somewhat rare phenomenon of vorticose currents. The examination of this spot began on the 20th instant; but before giving the details of this locality, I would make the following extracts from my note-book respecting three other days' observations, which, although disconnected in themselves, bear indirectly upon our present subject, in showing the apparent connexion of *ascensional* forces with the formation of solar maculæ.

1870, August 31. Near the Sun's western periphery a fine spot; the photosphere which formed the external boundary of the penumbra was marked by a circumscribing ring of elevated ranges, and the penumbra itself appeared to consist first of an uniform surface, and then, at the *margin* of the umbra, to rise again in upcast masses. The image of the solar disk thrown upon a white screen, and thus enlarged to two feet diameter, exhibited very clearly the same striking areographical features.* Here were evidently *upward* movements acting not only on the neighbouring photosphere, but also within the penumbral portions in immediate contact with the umbra.

Sept. 2. Another large spot; pear-shaped and possessing a very broad and flat penumbra; shut off light by diaphragms;

* Aperture of the Newtonian telescope used was nine inches diameter, which thus ensured a good amount of light for such enlarged views; and the same aperture was also used for direct vision, by excluding the intensity of the calorific rays (thus focalized), not only by coloured glasses but by diaphragms with differently drilled openings, taking care not to introduce serious optical errors by *too minute* perforations. The advantages of large apertures in solar observations are duly noticed in the valuable contribution to solar physics by Messrs. *De La Rue*, Stewart, and Löewy.

details well seen. Great inequalities were visible in the penumbral surface, quite similar in character to a disturbed region of the general photosphere, and differing only in light-reflecting powers.* Here also, therefore, appeared evidences of *upward* movement in the penumbra itself.

Sept. 8. Definition very good, Spots now few, but the photosphere strikingly corrugated. The undulations of light and shade dichotomizing and anastomosing in every direction, like a sunlit sea agitated by a strong stiff breeze.† Again struck with the general elevations *around* the solar spots.

Thus the photosphere around the openings, the actual penumbral surface, and even the penumbral coast-line bounding the umbra, equally exhibit apparent evidences of the evolution of forces from beneath, while in a former communication to the Astronomical Society I noticed evidences of *upward* movement over the umbra itself.‡ These phenomena seem unitedly to militate against the acceptance of the theory of a "downward current" of luminous matter (over umbræ), adopted by such high authorities as to make less experienced and qualified observers hesitate in suggesting a diverse opinion on this difficult point. Yet the designation of "craters" given to these maculæ by some solar physicists (if given in reference to their shelving and more or less approximately inverted conical form) would rather lead to a confirmation of the inference that such crateriform orifices resulted, as in volcanic agencies, from the gradually widening (or radiating) action of a subjacent focus of explosive energy.

I will now come to the consideration of the spot fixed upon for special and continued observation.

Sept. 20. A magnificent elliptical opening on the east limb, exhibiting, as before stated, evident symptoms of active elevatory movements. Its major axis (including penumbra) was not far short of 40,000 miles. Of this locality I had unusually distinct views during four successive days. I marked three straight promontories or tongues (A B C), which appeared attached to the inner north-eastern and northern margin of the penumbra, and projected at different lengths over the central umbra. Their relative positions were towards the N.E., N. and N.W. respectively. The most easterly (A) was very acuminated with a gradually widening base. The next in position (B) had a sharp, but rather short terminal, and continued towards its base with parallel sides. The other (C) towards the west was triangular in form. The apices of C and its neighbour B were near, but not in contact, while A was at some distance from B.

Sept. 21. To-day observed that C (about half way down)

* The light-reflecting powers of the penumbræ are essentially very great and strikingly observable when viewed through diaphragms, and so *uncontrasted* with the splendour of the general solar surface. The intense brilliancy of the calcium light becomes relatively a black spot when projected on the Sun's disk.

† More poetically—ποντίων τε κυμάτων ἀνήριθμον γέλασμα. (Æsch. Prom.) A similar scene is described by Mr. Huggins as "not unlike the stormy sea."

‡ *Monthly Notices* for June, 1869.

approximated in shape to a reaping-hook bent towards the west. The basic parts were seen to be intruded upon by yesterday's penumbra, and the penumbra in its turn was invaded by an advance (eastward) of the photosphere, which was abutting against the middle of the western side of the ellipse. The whole of B was curved in the same westerly direction, and the apex of A (on the N.E. side of the ellipse) had assumed also a bent form towards the west. With the diaphragm the terminal of C was observed to be bifurcated, with the westerly branchlet carried also towards the west. Since yesterday B had become narrower throughout its length, drawn out by the curvilinear motion. The western penumbra, forming the base of C, assumed also a curved shape towards the west. Very great disturbance visible on the south and south-easterly sides of the ellipse.

Thus evidence appears to be afforded by these two days' observations of the commencement of vorticose currents in the penumbra and its adjuncts, with a direction from right to left, forming a curve in the upper division of the ellipse, and the set of the photosphere from west to east.

Sept. 22. Definition exceptionally good. The large elliptical penumbra was now perceived to be converted approximately into two circular ones, which for convenience may be termed northern and southern divisions. C is gone, swallowed up by the advanced curved motion of the western penumbral masses. The greater part of B is now become straight, and only towards its southern apex does it still curve towards the west, making part of the southern circular boundary of the northern division, while the bases of the tongues (A and B) appear to have merged to form a portion of the south-east circular coast of the southern division, and curved eastward. Presumptive evidence of another vorticose movement east and south-east of the former.

The straight (and recently attenuated) part of B was more luminous than even the general photosphere, and I considered it to have been floating at a very high elevation, and thus out of the circular movement of the lower current, while the bent part of it (towards the west) partaking more of the penumbral tint (inferentially at a lower level), together with the basis of A and B (attached to the northern penumbra), would thus be under the direction of the lower current. There appeared to be great activity about this locality, and at good moments of observation I could perceive two wisps of the penumbra like windy cirri; one in its divided drawn-out parallel form, seemed to be a portion of the boundary of the northern part of the lower umbra, and the other normally curled and drawn in towards the central parts—indications of lateral and vorticose currents.

On the surface of the eastern penumbra great elevations were visible, with peaks as brilliant as the straight portions of B before mentioned, and the photosphere on the east was full of large billows, while round the margin of the penumbra the photosphere also showed similar forms, both by direct and reflected

vision. A delicate line (D) on the east of the larger part of the straight B (and projected like B from the margin of the northern penumbra) was visible when the solar glare was excluded by diaphragms and the view limited and localized.

Sept. 23. Definition very good ; the wind still easterly. The straight tongue B, with its apex turned yesterday to the west, was now found to be turned to the east, and the high photosphere of the west had apparently glided down and set in *en masse* for a *curved* flow towards the east, as the western penumbra at the base of C had previously curved towards the west. The delicate line D is still on the east of B, and not altered either in position or size—a sort of stratus-cloud indicating complete calmness locally, while the lower strata are bent round towards the east; showing that vorticose currents had extended so far towards the east, and indicating also both the existence and direction of the motion at lower levels, where the nebulous or gaseous matter was broken up into *curved wisps of smaller diameters.*

Now on the photosphere, all around what was the original and unbroken penumbra (and embracing both its subordinate and included ones), elevated ranges were to be seen whose topmost ridges and apices reflected the same degree of splendour as the northern part of B, and like the penumbral eminences of the previous day. I also noticed that one of these ranges on the easterly coast was bifurcated, and while one branch followed the trunk-range which formed the upper eastern border of what was the original great ellipse, the other branch turned off and took a *curvature* inwards, and formed part of the curved portion which dimidiated the ellipse.

These two days' observations will have led us to infer that a calm pervaded some of the higher regions, while energetic movements were going on in the lower, and that gyratory motions in a *contrary* direction had commenced in the eastern locality, and (on scrutinizing the directions and size of the circular forms) it also appeared that such curvatures had contracted their diameters, and that those on the north-west quarter had taken a direction from right to left, while in the south-eastern quarter the direction was from left to right.

Sept. 24. Found B still curved towards the east, but now more throughout its length, while the delicate line D was still unaltered. In this locality I could also make out, on careful survey, a minute fleecy line attached to D. This more diminutive object only became visible when the Sun's direct surface-brightness was shut out by diaphragms, conjointly with the exclusion of all indirect and refracted daylight by enwrapping (photographer-like) both head and telescopic eye-piece in an impervious India-rubber cloth. This (when thus viewed) exhibited interrupted brilliant patches, like the cusps of the early Moon, with their outlying enlightened highlands and intermediate unenlightened valleys, and, therefore, would lead to the inference that this solar branchlet occupied different levels at different points; in fact, was

of an undulating form.* I should add also, that this connected projection was clearly attached to the northern penumbra, and towards its southern portions it displayed photospheric brightness. It must thus have been an *undulating incline* rising up from the lower penumbral to the higher photospheric level. It might also be remarked that, while horizontal currents alone, when acting upon a gross material, would indeed produce an undulatory movement without the aid of ascensional currents (because in such a case the varying outline would be the *resultant* of horizontal and gravitating forces), yet as the rarity of the gaseous envelope precludes the idea of density sufficient for such effects, and we have already clear evidence of the tilting up towards the south of this luminous line, we may also with less scruple infer that this upward and undulatory movement might be the result of intermittent or spasmodic action from beneath.

Great changes, however, had been going on elsewhere. The body of the photosphere had rapidly set in from the elevated regions on the west and north-west and overspread the central portions of the divided ellipse, and had carried back A towards the east to straightness.

Now just south of this locality were portions of the photosphere to be seen overhanging the penumbral cliffs in a jagged outline, and projecting horizontally to a greater or less extent over the subjacent penumbral talus. The further development of a somewhat similar scene was witnessed by Pastorff in 1828. These features seem to suggest an existing *lateral* motion in high solar regions. The general and now complex currents had resulted in the breaking up of the upper half of the original ellipse into three compartments and of the lower into four.

These grand movements of the photosphere were however, I conceive, no more the actual *initial* cause of the vast changes thus superinduced over the once elliptical area than is the slow and irresistible march of the giant glacier the initial cause of its destructive consequences. Both are only proximate causes, and are themselves the results of a deeper and more hidden power. In the present instance a range of circular orifices on the western regions, and eruptive symptoms of a similar nature on the eastern, left no doubt in the mind of the observer as to the immediate source of the great photospheric disturbance now in progress.

Sept. 25. Outbursts more developed around the entire district, and while the general shape of the original ellipse (now divided and subdivided) is still marked, yet the connexion of the *sequence* of these changes could not now be traced, and therefore ceased to be of real instructive value.

Sept. 26. Disturbing causes became still more visible and general. The northern division now contains only two openings

* Large Newtonian Reflectors have decidedly great advantages over large Refractors for direct observations of the Sun, as the eye-piece, being placed at right-angles to the tube, allows the observer so to manage his dome-shutters as always himself to be in the shade.

and the lower three. The lower division is also completely severed, not only by the intrusive photosphere, but by an almost circumscribing chain of larger orifices, which eventually increased, and produced a virtual detachment of the two divisions. I noticed a long and thin tongue in the northern penumbra, but I could not venture to identify it with any previously existing one; and the definition to-day not being so good, I was also unable to speak decisively either as to its absolute connexion with the opposite penumbra, or as to any variation of brightness throughout its length.

In the present state of heliographical knowledge I would rather note what appeared to be observed facts respecting photospheric currents than assume any theory about their hidden and originating causes, whether intrinsic, from the expansion of gaseous matter; or extrinsic, from disturbing planetary attractions; or from the conjoint action of both such influences. In the chronological account of the last described solar district and its successive changes I have not met with any indications of a *downward* current over the umbra. But as the evidence in this particular case is simply of a negative nature, no general inference can be drawn against the possible occurrence of such at other times and over other spots. The gyratory movements described above were, I think, truly cyclonic in the strict meteorological acceptance of the term.

Every gyratory movement in our atmosphere is not indeed cyclonic, for such may arise from the action of lateral aerial forces proceeding from opposite directions, and producing whirlwinds combined with ascensional currents. Where, however, we have reason to infer that ascending currents are the *cause*, and not the *result* of such motion, then we may safely term such currents cyclonic.*

Now we have given (apparent) proofs in the earlier part of this Paper of the existence of *ascensional* currents over, and around, and within solar spots. We have also seen that the disturbance of the elliptical spot was in existence *anterior* to the development of the vorticose movement, so that with such an upward tendency of the Sun's envelope, combined with his own axial revolution, we have the two essential elements for cyclonic currents on an extended scale.† We have also noticed the apparent decrease of the diameters of these vorticose movements in the lower regions, which would seem to indicate an inverted conical or funnel-shaped disturbance, and likewise a revolving of such in opposite directions. These solar phenomena impress the mind

* "Whirlwinds are whirlwinds of compression, whereas cyclones are whirlwinds of rarefaction." See Sir John Herschel's masterly article on Meteorology in the *Encyc. Brit.*

† The velocity of rotation at the Sun's equator is 4415 miles per hour, while that of the Earth is only 1019 miles. (See Revised Tables in Donkin's "Astronomy.") Terrestrial cyclones are thus upon a comparatively small scale. Ordinarily from 2 to 300 miles diameter, and only sometimes exceeding 500 miles.

with the idea of an existing analogy between terrestrial and solar meteorological laws, and the idea (as suggested by Sir John Herschel) is strengthened by the well-known arrangement of the great display of macular zones on either side of the Sun's equator and by the freedom (comparative or absolute) from such photospheric outbursts on other parallels—corresponding so much with the presence or absence of cyclonic and extensive aerial disturbances in our own atmosphere. On the other hand, we find that the *reversed* motions of terrestrial cyclones occur respectively in the northern and southern hemispheres (separated as they are by the equatorial belt of calms),* and have a normal direction, north of the Equator from right to left, and south of the Equator from left to right, while in the above-described solar cyclones both appeared to move in reverse order, although in the same (northern) hemisphere.

Now under all these physical circumstances I am led to think that the due consideration of such analogies, and of any apparent exceptions, may become a means of drawing important inferences respecting the position of the source of solar disturbances. If cyclonic movements be carefully watched and described, and it should result, after a long period of observations, that there is a normal direction for such movements (*i.e* reversed in the different solar hemispheres), and that the vorticose movements around two centres (observed in the elliptical spot) have been an exceptional case or mistakenly described,—then we should rather infer that the grand comprehensive *influence* was from *without*, and operating upon the general surface of the photosphere. But if, on the contrary, the cyclonic movement should prove to be abnormal as compared with the terrestrial, and it should appear that I have not misinterpreted the changes observed, then we should be led rather to infer that such influence was from *within*, operating locally and not generally.

Yet contemporaneously with such observations we should also, I think, always direct our attention to the unbroken expanse of the photospheric ocean, and scrutinize those extensive and high heapings-up whose surface may never be scarred by any intrusive macular openings, and where, indeed, we should most probably be watching the very results of *external influences.* Above all, we should remember that not by hastily drawing inferences from one source exclusively, but by judiciously weighing and harmonising the evidences suggested by all the phenomena, can we even reasonably hope to attain to any valuable insight into the causes of the disturbance of the solar envelope, and be led to discern whether such be really simple or be not rather complex.

Note on Solar Faculæ.—The deep valleys beneath my Observatory are occasionally filled with a dense stratus-mist whose *upper* portions are subsequently converted by a cloudless Sun into every variety of the nebular forms except the "*nimbus.*"

* Vide "Physical Geography of the Sea," by Com. Maury, U.S.N. passim.

In the central parts of the valleys where the mist attains a depth exceeding 500 feet the first movement visible is a ridging or furrowing of the stratus producing the "*cumulo-stratus.*" When these furrows are most numerous and inosculating, and are viewed reflecting from their *upper* surface their dazzling sun-lit brilliancy and light-shadows, I recognise at once the most exact terrestrial counterpart of the solar photospheric *faculæ*.

Ensleigh Observatory, Lansdowne, near Bath,
27 *Sept.* 1870.

Note on the use of Eye-screens in Telescopic and other Researches. By Richard A. Proctor, B.A.

Most observers with the telescope keep the unemployed eye open, while studying with the other the features of an object in the telescopic field of view. Closing the unemployed eye produces, in fact, palpably injurious effects. But I suppose every one must have noticed how much better the features of a faint object are seen, when with the hand, or any screen, the unemployed eye is protected,—even though it be only from the luminosity of the star-lit sky, I find that much better results are obtained when the unemployed eye is guarded from the access of all light whatever by a screen lined with black velvet. Yet better vision is secured if the screen be so contrived as to guard the employed eye, also, from all light except what reaches it from the object. This can easily be managed by having a screen covering both eyes, but with sliding doors, by which a small circular aperture opposite either eye can be opened or shut at will. Each aperture can have outside it a small tube passing over the eye-tube of the telescope. Or (preferably for some purposes), a small sleeve-curtain with an elastic circular opening at one end can be attached, at the other, over either eye-aperture. Then if the opening be passed over the eye-tube and suffered to close by its elasticity, the desired result will be secured.

From my own experience I feel confident that for certain special orders of research the use of eye-screens of this sort (the details being arranged according to convenience) will be found of great service. I may particularise—observation on moonlit nights, the examination of faint nebulæ or the search for them, the search for minute points of light, delicate spectroscopic and polariscopic researches, and the investigation of questions concerning the colours of the planets, of the lunar regions, &c. The following description of the *modus operandi* will serve to supplement the above general sketch. The equatorial being set on any delicate object, the observer fastens the screen over his eyes, attaches the sleeve curtain over the eye he proposes to work with, slides the elastic ring over the eye-tube, and suffers it to close round that tube. Having seen that the object is in the

field, and the rate of driving just, he closes both the eye-doors, keeping his eyes open and directed on the perfect blackness of the velvet lining. When he thinks the eye sufficiently prepared, he draws out the sliding eye-door over the telescope-eye, and is then able to apply the full powers of his eye-sight (under exceptionally favourable conditions) to the examination of the object he has to deal with.

I am desirous of learning how far the efficiency of such contrivances depends on aperture,—that is, whether the performance of large telescopes is as much improved as that of smaller instruments. If any Fellows of the Society should care to experiment on this matter (which is not unimportant, I conceive) they would be conferring a favour on me by communicating the results they may obtain. I refer here simply to the comparison between the work of a telescope used in the ordinary way and when supplemented by eye-screens.

I may renew here my suggestion of the great importance of applying such contrivances during the examination of the corona in total solar eclipses, whether with the telescope, spectroscope, or polariscope. As regards the spectroscopic analysis of the corona, any method by which the visibility of a very faint continuous spectrum can be increased (and I imagine that eye-screens such as I have proposed would have such an effect) cannot but be well worth trying. It seems highly probable that the variations in the accounts given so far depend wholly on the amount of opening in the slit,—a narrow slit giving a very faint continuous spectrum and three somewhat brighter lines (one can scarcely speak of bright lines in this case) which are under these circumstances very difficult and delicate objects of observation; a somewhat wider opening gives a continuous spectrum, bright enough to obliterate two of the lines, but leaving one (the one Professor Harkness saw) fairly visible; a wider opening shows only a continuous spectrum, the lines being all obliterated. This at least is the explanation which seems alone consistent with the principles of spectroscopic analysis, and it is obvious that if it be just, any increase in the eye's power of appreciating faint light must be a great gain.

On a Point of Egyptian Chronology. By C. Abbe, Esq.

My limited acquaintance with the recent researches into ancient chronology, and with the standard works on this subject, necessitate my stating that the object of the present Note is to direct more exact attention to a subject that has been touched upon in a general way by others.

In the *Monthly Notices*, vol. xxiv., Mr. Williams gives a list of ninety-two eclipses observed in China from 720 B.C. to the year 0. *These we* may assume to have been observed in various parts of

the empire, with the unaided eye, and without having been approximately predicted; and may be taken as a fair exhibit of the result of similar observations made by the astronomers of other ancient nations.

Bunsen, in his *Egypt*, vol. i. p. 14, quotes from Diogenes Laertius (who flourished about the year A.D. 200), that the Egyptian records embraced 373 solar and 832 lunar eclipses. By comparing this with the preceding statement of Mr. Williams, we may arrive at an estimate of the number of years that must have elapsed since the beginning of the Egyptian record, assuming only that the climate and other conditions equally favoured both the Chinese and the Egyptian observers.

A simple proportion gives for the 373 Egyptian eclipses a period equal to 4·05 times the 720 years of the Chinese record, or about 2920 years.

It is not probable that the records of the Egyptian astronomers were seriously interrupted by the wars and conquests, and as the Ptolemies fostered our science, I should esteem it probable that the lists quoted by Laertius were complete up to his time, this gives us B.C. 2720 for the beginning of the record. This date would be put back still earlier if the Egyptian records were imperfect.

Now Bunsen puts the seven years' reign of Asses or Aseth at 2782 to 2775 B.C., in whose time the Egyptian year was instituted. It may then be surmised that from his time forward a record of eclipses was kept as unbroken as possible, although it would have appeared more plausible that such record should have begun 276 years earlier, *i.e.* in the reign of Meves. A detailed study of the eclipses visible in Egypt, and the exclusion of those that were too small to have attracted notice, would settle this point.

I have made the following attempt to determine the *minimum* interval required for the accumulation of the Egyptian observations. The *Nautical Almanacs* from 1834 to 1870 (the only ones accessible to me) show that there were during that interval 86 solar and 57 lunar eclipses, of which respectively 12 and 30 were visible at Alexandria. Comparing these with the statement of Laertius, we deduce two independent determinations of the interval, *i.e.* respectively 1120 and 998 years. It also appears that there were about 240 solar eclipses in the 720 years, within which the Chinese recorded only 92.

It would be interesting if a comparison could be instituted between the lunar eclipses enumerated by Laertius and those of the Chinese.

Cincinnati,
August, 1870.

Observations of the Occultation of Saturn by the Moon, Sept. 30, 1870. By C. L. Prince.

I observed the occultation of *Saturn* by the Moon this evening under favourable circumstances, with the exception of the presence of an easterly wind, which always interferes, more or less, with sharp definition. As the Moon approached the planet a very striking difference was observable in their relative brightness; and this difference was still more marked at the time of reappearance when the planet assumed a very pale green colour.

At the moment of contact, as well as throughout the occultation, there was not the slightest distortion of either body; but I noticed that the edge of the ring lingered somewhat upon the Moon's limb about the time of disappearance. The ring, however, maintained a well-defined edge to the last.

The following times of immersion and emersion are tolerably correct, considering the prevalence of an easterly wind, and the very low altitude of the planet,—

Immersion.	h	m	s	
First contact with ring	18	41	28	L.S.T.
Disappearance of ring	18	41	45	
First contact with ball	18	41	53	
Ball occulted	18	42	30	
First contact with inner edge of ring	18	42	37	
Disappearance of ring	18	42	55	

Emersion.	h	m	s	
Reappearance of ring	19	55	18	L.S.T.
Reappearance of ball	19	55	42	
Reappearance of following limb	19	56	14	
Reappearance of inner edge of ring	19	56	22	
Latest contact	19	56	39	

I employed my Tulley Equatoreal of 6·8 inches aperture, and 12-feet focal length. Power 250.

Observatory, Uckfield,
Sept. 30*th* 1870.

Observations of Coggia's Comet. By S. J. Perry.

The comet discovered this year by M. Coggia of Marseilles has been observed here during the month of October, and the *following* positions determined,—

Oct.	G.M.T.	Star of Comparison.	R.A.	N.P.D.	Comet $d\alpha$	$d\Delta$
			h m s	° ′ ″	m s	° ′ ″
2	12 50 A.M.	*α Andr.*	0 1 42·98	61 37 21·9	+ 9 6	+1 8 0
2	10 9 P.M.	"	"	61 37 21·7	+ 3 16	+0 51 20
3	9 5 "	"	"	61 37 21·5	− 4 10·5	+0 20 10
4	7 25 "	"	"	61 37 21·3	−11 58	+0 0 45
9	7 50 "	Star *a*	23 14 30·3	60 17 42	+ 1 50	+0 8 0
9	11 35 "	"	"	"	+ 1 18	+0 6 40
10	10 39 "	Star *b*	23 9 56	60 0 4	− 0 32	+0 15 0
14	8 42 "	Star *c*	22 50 30	59 50 36	− 1 15	+0 0 15

The Comet was also observed on the 20th, but as only an approximate position could be obtained on account of the badness of the weather, I have not entered the result. Throughout the latter half of the month the clouds have rendered observations almost impossible.

Stars *a*, *b*, *c*, are taken from Argelander's large maps, and have been reduced to Oct. 1870. Refraction does not sensibly alter $d\alpha$ and $d\Delta$.

The concluded positions of the Comet are

	G.M.T.	R.A.	N.P.D.
	h m	h m s	° ′ ″
Oct. 2	12 50 A.M.	0 10 49	62 45 22
2	10 9 P.M.	0 4 59	62 28 42
3	9 5	23 57 32	61 57 32
4	7 25	23 49 45	61 38 6
9	7 50	23 16 20	60 25 42
9	11 35	23 15 48	60 24 22
10	10 39	23 9 24	60 15 4
14	8 42	22 49 15	59 50 51

Stonyhurst Observatory.

The Laws according to which the Stars Visible to the Naked Eye are distributed over the Heavens. By Richard A. Proctor, B.A. (Abstract.)

The method briefly described in the Supplementary No. of the *Monthly Notices*, vol. xxx., has led me to the following general results.

I find that the Milky Way, both in the northern and southern hemisphere, is singularly rich in stars, and in each hemisphere severally it is the richest special region of all. In the northern* there is a very rich region covering *Cygnus*, *Cepheus*, *Ursa Minor*, and *Lacerta*. It is numbered 2 in the accompanying Table. In the southern hemisphere there is a corresponding rich region

* Several copies of a map illustrating the distribution of all the lucid stars over the heavens were exhibited at the Meeting. The map belongs to the second edition of my "Other Worlds."

covering the keel of *Argo*. It is numbered 12 in the Table. But nearly half the northern heavens (the more central portion) is exceptionally rich in stars, while even in a more marked degree one half of the southern heavens (almost centrally surrounding the Greater Magellanic Cloud) exhibits exceptional richness. These are numbered 3 and 11 in the table. The remaining portions of the two hemispheres form the parts numbered 4 and 10 in the table. Certain parts of this outer and poorer region are exceptionally poor in lucid stars. They abut on the richest parts of this rich region, and the contrast is thus rendered more striking. They are numbered 5, 6, 8, and 9 in the accompanying table. It will be noticed that none of the areas included in the table are very small, except only those forming gaps and lacunæ in the Milky Way. For convenience many of the regions have been limited to a certain size, viz., $\frac{3}{22}$nds of the hemisphere.

	Name of Region.		Area. Area of Hemisphere as 1.	Number of Lucid Stars.	Richness. Average=5850.
Northern.	1 Milky Way	..	$\frac{1}{11}$	497	9940
	2 Richest region	..	$\frac{3}{22}$	622	9050
	3 Central region	..	$\frac{5}{11}$	1420	6248
	4 Outer region	..	$\frac{6}{11}$	1070	3923
	5 Poor region I.	..	$\frac{3}{22}$	201	2948
	6 Poor region II.	..	$\frac{3}{22}$	175	2567
	7 Gaps in Milky Way	..	$\frac{1}{62}$	20	1240
Southern.	8 Poor region I.	..	$\frac{3}{22}$	161	2361
	9 Poor region II.	..	$\frac{3}{22}$	216	3198
	10 Outer region	..	$\frac{1}{2}$	893	3572
	11 Central region	..	$\frac{1}{2}$	2467	9868
	12 Richest region	..	$\frac{3}{22}$	895	13126
	13 Milky Way	..	$\frac{1}{10}$	618	13596

The numbers in the last column indicate the total number of stars which would be visible to the naked eye, if the whole heavens were covered as richly or as sparsely as the corresponding region.

This Table proves that special laws of aggregation and segregation exist among the lucid stars. The fact that the Milky Way is also shown to be exceptionally rich in lucid stars, while its gaps and vacuities are exceptionally bare, seems to dispose beyond all question of the theory that the stars forming the milky light of the Galaxy (regarded as a whole) lie at vast distances beyond the lucid stars.

It may be mathematically demonstrated that even the less marked feature, the exceptional richness of the southern hemisphere, cannot be ascribed to chance distribution; thus—

The northern heavens contain 2490 lucid stars, the southern

3360, or 5850 in all (we need not inquire whether these numbers are exactly correct, because the laws of probability include chance errors as well as chance distribution). The probability that a star placed at random will lie on one or other hemisphere equals $\frac{1}{2}$. Hence the antecedent probability that out of 5850 stars so distributed as many as 3360 will be found in either hemisphere is represented by a fraction whose numerator is the sum of all the coefficients in the expansion of $(x+y)^{5850}$, in which either x or y is raised to a power of not less than 3360; and its denominator the sum of all the coefficients, or $(2)^{5850}$. The sum of the favourable coefficients is

$$=2\left[1+\frac{5850}{1}+\frac{5850 \cdot 5849}{1 \cdot 2}+\text{\&c.} \ldots \ldots +\frac{5850 \cdot 5849 \ldots \ldots 3361}{1 \,.\; 2 \,\ldots\, 2490}\right];$$

and the sum of the unfavourable coefficients is

$$=\frac{5850 \cdot 5849 \ldots 2926}{1 \,.\; 2 \ldots \ldots 2925}+2\left[\frac{5850 \cdot 5849 \ldots 3360}{1 \,.\; 2 \ldots \ldots 2491}+\frac{5850 \cdot 5849 \ldots 3359}{1 \,.\; 2 \ldots 2492}\right.$$

$$\left.+\text{\&c.} \ldots \ldots +\frac{5850 \cdot 5849 \ldots 2927}{1 \,.\; 2 \ldots 2924}\right].$$

The total number of favourable terms is 4982, that of the unfavourable 869, or more than one-sixth of the former. Hence by comparing the largest unfavourable term with six times the largest favourable term, we shall get a ratio obviously less than that we require. (It needs only a consideration of the law according to which the coefficients increase to see this.) Hence the odds against the occurrence of the observed arrangement of the stars, as respects the northern and southern hemispheres, would be, if chance-distribution were alone in question,

$$> \frac{1}{6}\left\{\frac{\dfrac{5850 \cdot 5849 \ldots \ldots 2926}{1 \,.\; 2 \ldots \ldots 2925}}{\dfrac{5850 \cdot 5849 \ldots \ldots 3361}{1 \,.\; 2 \ldots \ldots 2490}}\right\}$$

$$> \frac{1}{6}\left\{\frac{3360 \cdot 3359 \ldots \ldots 2926}{2491 \cdot 2492 \ldots \ldots 2925}\right\}$$

and obviously therefore (*à fortiori*)

$$> \frac{1}{6}\left\{\frac{2926 \cdot 3013 \cdot 3100 \cdot 3187 \cdot 3274}{2925 \cdot 2838 \cdot 2751 \cdot 2664 \cdot 2577}\right\}^{87}$$

Now the value of this expression is easily calculated to be

66,996,090,000,000,000,000,000.

Hence the antecedent probability of the observed event is less than

$$\frac{1}{66,996,090,000,000,000,000,000}.$$

The denominator would have had 132 figures had we dealt with the probability of the existence of two such regions as those numbered 3 and 11 in the above table.

As respects this last relation it may readily be shown that if *a sphere having a radius exceeding many million times the distance of the furthest object revealed by the Rosse Telescope, were filled with atoms severally minuter many million-fold than the minutest object which the microscope will reveal, the chance that a specified atom would be selected at random from that inconceivably vast universe of atoms, would be many million times larger than the antecedent probability of the observed relation, supposing chance-distribution alone in question. Yet that relation is by no means the most remarkable exhibited in the above Table.*

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXI.	*December* 9, 1870.	No. 2.

William Lassell, Esq., President, in the Chair.

Henry Barnes, Esq., The Moorlands, Clapham,

was balloted for and duly elected a Fellow of the Society.

On a Photograph of Jupiter. By J. Browning, Esq.

On the evenings of the 24th and 25th of October, which will be remembered for the magnificent displays of the Aurora Borealis—the air being steadier than usual,—I made two careful drawings of the planet *Jupiter*. As these were made on successive evenings,—the first on October 24, at 11 P.M. and the second on the 25th at 10.45 P.M., the drawings represent nearly the whole surface of the planet. The equatorial belt is of a fuller ochreish or tawny colour than when I last observed it—during the previous apparition. A bright belt to the north of the equator is much the brightest portion of the planet's disk. The dark belts on the northern side were of a very dark brown, with less copper colour in them than I found during my previous observations. The portion of the disk to the south of the equator was peculiarly free from belts. This refers especially to the views obtained on the 24th. The hemisphere seen on the 25th had a light and a dark belt about midway between the south pole and the equa-

tor and tolerably prominent. The ochreish belt was mottled all over the surface with white cloudy markings or patches—a distinct line of them, though separated by darker markings between, evidently encircled the whole of the planet, a little way to the south of the true equator.

I began a series of very careful micrometric measurements of the relative measures of *Jupiter*, to endeavour to determine whether any change had taken place in the proportions of the polar and equatorial diameters; owing to the very unfavourable state of the weather, I have not been able to complete these measurements.

Since writing the above, Lord Lindsay has shown me two photographic negatives of *Jupiter*, taken in Mr. De La Rue's Observatory on the same evening, within a quarter of an hour of the time I made my first drawing. It is worthy of remark that the equatorial belt in these negatives is almost absolutely transparent, the light from this orange-coloured belt having failed entirely to act on the sensitive collodion surface. I have seen negatives of *Jupiter* taken during previous years in which this equatorial belt had exerted the most action on the surface, giving the belt as quite opaque. I have appended a rough diagram of this negative to my paper. The periodicity of the change in the colour of the equatorial belt would be best determined by taking photographs of the planet.

In the plate A is a copy of the first coloured drawing of the planet taken on October 24th at 11 P.M., and B is a copy of the photographic negative, as seen with a microscope, taken within a few minutes of the same time.

It seems to me probable that a careful examination of photographic negatives of *Mars* might throw some light on the disputed question of the colour of the darker portions of the planet. We have evidently not yet exhausted the applications of photography to the cause of astronomical research.

On Periodical Changes in the Physical Condition of Jupiter. By A. C. Ranyard, Esq.

During the past year, the changes which have been go forward on the surface of *Jupiter* have excited the att tion of many observers. Several drawings have been shown meetings of the Society exhibiting strong marks of colour usually seen, and other drawings evidently very carefully exe show bright egg-shaped patches lying along the central bel by Prof. Mayer of Lehigh University, U.S., shows a r elliptical marking among the southern belts,—the same m was observed in England by Mr. Gledhill, at Mr. C Observatory.

A similar increase of colour and bright egg-shaped

A

B

JUPITER.

A. From a drawing
by JOHN BROWNING.
B From a Photograph.

were observed in the years 1858, 1859, and 1860: Mr. Huggins, Mr. Lassell, Mr. Airy, and Sir W. Keith Murray, all noticed and figured them, their drawings in many respects correspond with those made in the course of the last season. The oblique markings described by Mr. Airy in 1860 have also been again observed, though only in a modified form, during the last epoch by Mr. Webb, of Hardwick, whose drawings in all other respects greatly resemble those by Mr. Huggins made in 1858–9.

It will be noticed by those interested in the periodicity of Sun-spots, that these two periods of maximum change on *Jupiter's* surface agree with two periods of maximum development of spots on the Sun's surface.

It becomes, therefore, interesting to inquire whether similar changes on the planet have been noted and figured at other periods of maximum solar disturbance. I have searched the *Index to the Monthly Notices*, recently prepared by Mr. Williams, and can only find one account of white spots being seen upon *Jupiter* previously to the year 1858.

This notice of them was at the beginning of 1850, when they are described by Mr. Lassell as having been "most remarkable." Mr. Dawes was at the meeting when the paper was read, and said he had seen the same appearance. There was a period of maximum solar disturbance in the year 1848, and both the magnetic and solar disturbances continued to be very considerable up to the year 1850.

The *Monthly Notices* of the Society do not extend back as far as the period of maximum solar disturbance which occurred previously to this in 1836–37.

I have at present had no opportunity of making any extended examination of observations of *Jupiter* taken previously to the commencement of the *Monthly Notices*, but the few accounts of white spots which I have already collected all occur near to some some period of maximum development of Sun-spots.

A table of such maxima since the year 1615 is given by Dr. Wolf, of Zürich, in the twenty-fourth number of his *Astronomische Mittheilungen*. I may especially mention some observations of Sir W. Herschel of white spots and irregular bands in 1778, 1779, and 1780—as well as the fact that he noticed what he called a "similar appearance" in June 1790. Two very considerable magnetic maxima occurred, the one in 1779·5, and the other in 1788·5, the last was a long while in subsiding, in fact there is a second and subsidiary maximum registered for 1792. Schroeter also notices the remarkable changes which took place on the surface of the planet in the winter of 1786.

The next three magnetic and solar maxima are very inferior in point of magnitude to the maximum of 1788–9, and if such slight negative evidence be of any value, I may mention that at present I have found no notice of white spots during the period from 1795 to 1830.

In the *Mémoires de l'Académie* for 1692, there is a paper on

Jupiter by Cassini, in which he notices great changes and bright spots on *Jupiter* about that time.

Dr. Wolf gives a Sun-spot maximum for 1693 ± 2.

There is a most interesting remark of Cassini's in this paper, especially when it is taken in connexion with the proper motion of spots upon the Sun's surface. He observed that the bright markings upon *Jupiter* had a proper motion of their own, and that that motion was greater the nearer the spots were situated to *Jupiter's* equator. It would be interesting to inquire whether this has been again noticed by Mr. Airy and other observers who have since attempted to determine the time of *Jupiter's* rotation.

If a future more complete examination of the observations of *Jupiter* should confirm the suspicion that the Sun and *Jupiter* have the same period of maximum disturbance, it would appear to show that the alternations on *Jupiter* are dependent upon some cosmical change, and not on any effect of tides as suggested by Dr. Wolf in the case of the Sun.

Automatic Spectroscope for Dr. Huggins' Sun Observations. By Howard Grubb, Esq., C.E.

The spectroscope as exhibited is in an unfinished state, having been sent to Dr. Huggins for arranging some small matters of convenience, such as the dividing of Sector, Reading microscope, &c.

It consists of a combination of 4 compound prisms and 2 semi-compound prisms, all made use of twice, the total power of the instrument therefore being equal to 10 compound prisms, each having a dispersion of about 9°, that is, a total dispersion of about 90°, probably the largest ever obtained. The observing and collimating telescopes are respectively 6 and 4½ inches focus, and 1 inch aperture, the section of pencil actually in use being 1 inch by : 0·6 inch. This is perfectly constant from end to end of spectrum, as the prisms are automatically worked.

The prisms are 2¼ inches high, being just twice the height required for the section of pencil: the lower half being made use of for the first course of rays, the upper for the backward course.

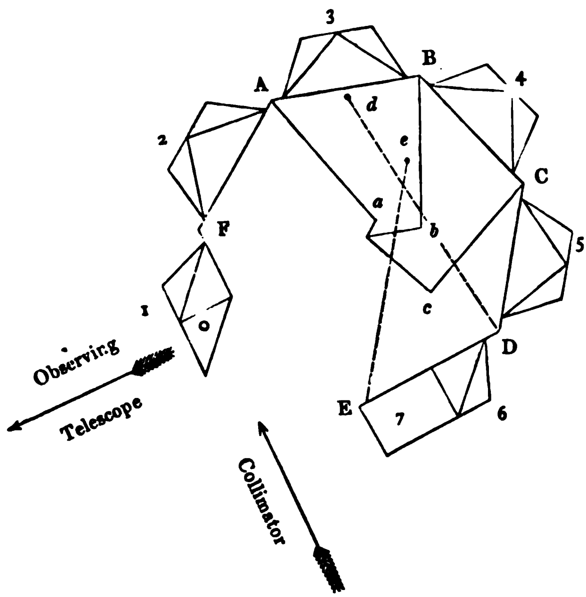
Referring to diagrams (the same letters of reference apply t*r* both) the dotted lines represent those levers, &c., which a situated in a different plane, being at back of spectroscope. T right-angle prism of reflection (0) is applied only on the up*ŗ* half of the first semi-compound prism (1), so that it does interfere with the first course of the rays which utilize on' lower half of the prisms thus,—

The parallel rays from the collimator enter the lo*v* of the first semi-compound prism without refraction, tl (1), therefore, is stationary. It then passes through fo compound prisms, 2, 3, 4, 5, and one semi-compound, 6, fr*r* *by two internal* total reflections in the prism of re

it is passed to upper half of prisms by which it returns through the four entire compounds and two semi-compounds, and is finally received emerging from the first fixed semi-prism by the right-angled prism of total reflection o, and so passed to the observing telescope which is placed at right angles to the collimator merely as a matter of preference. Any other position can be utilized if desired.

The prisms and automatic arrangement are contained in an air-tight box, and both observing and collimating telescopes are stationary, considerable advantages in such a powerful spectroscope, and allowing great compactness.

The several parts of the spectrum required to be examined are brought into the field by acting on the sector, which carries



the automatic arrangement, each line being exactly in minimum deviation when brought to centre of field.

The sector receding by a vernier to 10 seconds of arc divides the spectrum into about 20,000 parts.

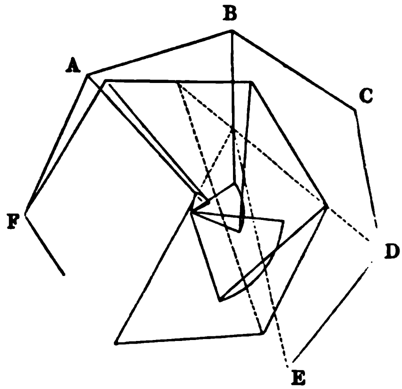
The mechanical arrangement of the automatic movement is that which we made a model of during Dr. Huggins' visit here last spring, and decided upon as giving the most constant and reliable results.

The motion is given to the chain of prisms entirely by a system of levers which will be easily understood from the diagrams.

The first three moveable joints of chain A B C are connected

by levers to the studs *a*, *b*, *c* fixed in a circular disc, which is rotated through 60° by toothed sector and pinion. The pins being fixed at their proper radii, draw the several prism tables through the required angle, the levers forming tangents in their mean position. The last two joints D and E were found geometrically to describe most accurately arcs of circles, they have therefore been attached to levers working on fixed centres at back of spectroscope, shown in the drawing by dotted lines.

The whole system of the automatic movement is composed of



hardened steel pivots, working in hardened steel bearings, a system which can obviously be made to work with the greatest accuracy and constancy.

The delicate steel parts of slit have been electro-gilt to preserve them from oxidation. The jaws (of gold plate) are drawn asunder by a double wedge, acted upon by a screw, so as to preserve axis of collimation. They are pulled together by a spring at back. The micrometer head of screw is divided into forty parts, each division being equivalent to $\frac{1}{40,000}$ of an inch of opening.

N.B.—The slots in table of spectroscope have nothing whatever to do with the guiding of chain of prisms. They are merely to allow of the junction of the two systems of levers working in different planes.

On a Standard or Uniform Measure of Time. By Lieut.-Col. Drayson, R.A., Professor of Surveying, R. M. Academy, Woolwich. (Abstract.)

The author considers that the Pole of the heavens, or axis of the Earth, describes a circle in the heavens during a long period of years, and that the only constant unit of time is, *cæteris pari-*

bus, the interval of time between two successive transits of the centre of the circle described by the Earth's axis—being, in fact, the mean of the intervals of the times of transit of any star within the circle, during an entire revolution of the axis. But as the obliquity is variable, the centre of the circle in question is not the pole of the ecliptic; and on examination of the evidence the author arrives at the conclusion that the centre is not (as he had somewhat hastily assumed) within a few minutes of arc from that pole, but that it may be stated to be 6° from the pole of the ecliptic, with, in the year 1860, a Right Ascension of about 268° 40′; and he finds that this result is consistent with the observed variation of the obliquity.

On the Graphical Construction of a Solar Eclipse. By Prof. Cayley. (Abstract.)

The celestial sphere is projected stereographically, in such manner that the Moon's centre is at the centre of the projection, and that the meridian through the Moon's centre occupies a fixed position. If, for a moment, we suppose that we also project the Sun's relative orbit, we have at any instant during the eclipse the position of the Sun's centre, and with this centre describing a circle the radius of which represents the sum of the semidiameters of the Sun and Moon: then, if Q′ be any point on this circle, M the centre of the projection, and we draw the straight line, Q′M, producing it to a point Z such that $Q'M = P' \sin ZM$ (where Q′M, ZM denote the arcs represented by these lines respectively, and P′ is the difference of the parallaxes of the Moon and Sun, or say it is the quantity thus designated in the *N.A.* for 1836), then we have the position of a point Z on the Earth's surface, at which the Sun and Moon are, at the instant in question, seen in exterior contact; or by considering the series of points Q′ on the circle, we have the locus of the corresponding point Z, or, what is the same thing, the penumbral curve on the Earth's surface. The apparent orbit is not actually constructed on the scale of the projection, but on a greatly enlarged scale, in such manner that the parallactic depression due to a zenith distance 90° is represented by the radius of the bounding circle of the projection. And the actual process of constructing the projection of the penumbral curve consists merely in the graphical transformation of a circle by means of a properly graduated straight edge: viz. placing this with its centre-point on M, and so as to pass through any point Q on the circle (centre the position of the Sun's centre, and radius the sum of the semidiameters of the Sun and Moon, as actually represented on the enlarged scale) we at once mark off, by means of the graduation, the required point Z, and so obtain the projection of the penumbral curve at any instant during the eclipse. It will be observed that the Moon being always at the centre of the projection, and there-

fore any terrestrial meridian, say the meridian of Greenwich, a variable line on the projection, the projections of the penumbral curve for the different instants during the eclipse are constructed, not in their actual positions, but as affected with a variable displacement in longitude: we read off from the figure the latitudes and longitudes of as many points as may be necessary on any one of these curves, and so transfer the series of curves to a map on any projection which may be convenient.

It is further to be noticed that the stereographic projection required for the construction is one wherein the distance of the pole from the centre of the projection is the polar distance of the Moon's centre at the time of the eclipse, viz. that a different projection is required for each eclipse; the construction of such a projection would be by far the most laborious part of the whole, but it is fortunately unnecessary; viz. if we have a blank projection, in which the distance is $= 90°$ (viz. the ordinary projection of an eastern or western hemisphere), but with the circles produced beyond the bounding circle, then we can, by simply drawing a new circle, cut out from it a projection in which the pole occupies the required position; see as to this the Note on a property of the stereographic projection, *Monthly Notices*, vol. xxx., p. 205. I constructed the blank projection, radius $= 12$ inches, and with the meridians and parallels at intervals of $5°$; and completed the construction for the eclipse of Dec. 21–22, 1870, bringing out the times and places of the beginning and end of the eclipse with an error of about $1\frac{1}{2}$ minutes of time, and $1\frac{1}{2}°$ of place; but I think that with a more complete blank projection, and more care in the construction, the limits of error might be greatly diminished; and that the construction, performed on the scale in question, would give results of as much accuracy as could be exhibited in a diagram such as the eclipse diagrams of the *Nautical Almanac.*

On the Proper Motions of 36 (A) *Ophiuchi and* 30 *Scorpii.* By W. T. Lynn, B.A.

Perhaps the most remarkable instance of stellar Proper Motion which has been detected is that afforded by two stars on the boundaries of the constellations *Ophiuchus* and *Scorpio*. One of these, 36 (A) *Ophiuchi*, is a double star, and a known binary, the two components being of about the 5th and 6th magnitudes respectively. The other, 30 *Scorpii*, is a single star of the 7th magnitude. The distance in a great circle between A *Ophiuchi* and 30 *Scorpii* is about $12' 21''$, and the remarkable point is that both appear to be animated by a large proper motion of the same amount ($= 1''\cdot27$ in a great circle) and in the same direction. This has been long ago determined, but I have thought it might be useful to calculate the value as deducible from the observations made in recent years at the Greenwich Observatory. The stars

attain a meridian altitude at Greenwich of only about 12°, which renders the observations of N.P.D. somewhat uncertain; but, as their number is considerable, the result is entitled to be considered pretty accurate.

The following is a table of the results of the observations, as given in the successive great Catalogues,* with the number of observations contained in each:—

		A¹ Ophiuchi.		A² Ophiuchi.		30 Scorpii.	
Authority.	Epoch.	R.A.	No. of Obs.	R.A.	No. of Obs.	R.A.	No. of Obs.
		h m s		h m s		h m s	
12-year	1840	...		17 5 31·05	2	17 6 23·37	5
12-year	1845	17 5 49·24	5	17 5 49·44	2	...	
6-year	1850	17 6 7·51	2	...		...	
7-year for	1860	17 6 44·47	12	17 6 44·66	9	17 7 37·18	8
7-year for	1864	17 6 59·27	3	17 6 59·33	1	17 7 51·80	2
		N.P.D.		N.P.D.		N.P.D.	
		° ′ ″		° ′ ″		° ′ ″	
12-year	1840	116 21 38·87	7	116 21 34·95	11	116 18 30·26	10
12-year	1845	116 22 4·83	1	116 22 0·64	1	...	
6-year	1850	116 22 36·76	3	...		...	
7-year for	1860	116 23 35·06	12	116 23 31·26	9	116 20 25·21	8
7-year for	1864	116 23 59·08	3	116 23 55·01	1	116 20 47·77	2

From these we obtain the following mean annual variation for each star:—

	In R.A.	In N.P.D.
	s	″
A¹ Ophiuchi	+3·689	+5·82
A² Ophiuchi	+3·675	+5·67
30 Scorpii	+3·675	+5·71

And, as the annual precession (for 1860) of 36 *Ophiuchi* amounts to $+3^{s}{\cdot}718$ in R.A. and $+4''{\cdot}62$ in N.P.D., and that of 30 *Scorpii* to $+3^{s}{\cdot}717$ in R.A. and $+4''{\cdot}54$ in N.P.D., it follows that the annual proper motion of each star deducible from the Greenwich observations will be:—

	In R.A.	In N.P.D.
	s	″
A¹ Ophiuchi	−0·029	+1·20
A² Ophiuchi	−0·043	+1·05
30 Scorpii	−0·042	+1·17

Greenwich, 1870, *December* 3rd.

* For the N.P.D.'s of the 12-year and 6-year Catalogues, the corrected values at the end of the 7-year Catalogue for 1860 are given.

On the Proper Motion of Oeltzen's Argelander 17415–6.
By W. T. Lynn, B.A.

This small star, which is of the ninth magnitude, was observed twice in the zone-observations of Argelander, and the two observations are numbered 17415 and 17416 in the Northern Catalogue formed by Oeltzen from those zones. Argelander had found that the star had a large Proper Motion, amounting to about $1''\cdot 2$ of a great circle. Subsequently Krüger made a determination of its annual parallax with the Bonn heliometer, and found it to be $0''\cdot 247$, with a probable error of $\pm 0''\cdot 021$ (*Monthly Notices* for 1863, March 13, vol. xxiii. p. 173).

The star was entered for observation in the Greenwich Working Catalogue for 1864 and subsequent years. Unfortunately the N.P.D. was given erroneously, the declination in Oeltzen's Catalogue being inadvertently copied as N.P.D., and this mistake was repeated from year to year, being only detected quite recently. The star therefore numbered 1974 in the Greenwich Seven Year Catalogue for 1864 is not Oeltz. Arg. 17415, as it is there called, but a star whose declination is nearly the complement of that of the star in question.

On August 12, however, of the present year, the star was observed with the Greenwich transit-circle, and the mean place deduced from that observation is, for 1870, Jan. 1,—

R.A.	$17^h\ 37^m\ 10^s\cdot 91$
N.P.D.	$21^\circ\ 32'\ 32''\cdot 30.$

Oeltzen's place for 1842 is,—

R.A.	$17^h\ 37^m\ 21^s\cdot 32$
N.P.D.	$21^\circ\ 31'\ 2''\cdot 40.$

The precession for 1870 is in R.A. $-0^s\cdot 298$, secular variation $+0^s\cdot 0136$; in N.P.D. $+1''\cdot 993$, secular variation $+0''\cdot 0424$. So that Oeltzen's place, geometrically reduced to 1870, would be

R.A.	$17^h\ 37^m\ 12^s\cdot 92$
N.P.D.	$21^\circ\ 31'\ 58''\cdot 04.$

From this we find the proper motion of the star to be

In R.A.	$-0^s\cdot 07$
In N.P.D.	$+1''\cdot 22.$

Applying the fractional part of this to the place deduced above from the Greenwich observation for 1870, August 12, we reduce the mean place for 1870, Jan. 1, to:—

R.A.	$17^h\ 37^m\ 10^s\cdot 95$
N.P.D.	$21^\circ\ 32'\ 31''\cdot 55$

And it thus appears that the accurate annual proper motion, as far as it can be determined from this observation, is:—

In R.A. $-0^{s}{\cdot}07$
In N.P.D. $+1''{\cdot}20$.

I am permitted by the Astronomer Royal to communicate the observation, from which this result is deduced, to the Society.

Greenwich,
1870, *Nov.* 15.

Observations of Coggia's Comet (II. 1870). By J. Joynson, Esq.

1870.	Waterloo Sidereal Time. h m s	R.A. h m s	Decl. ° ′ ″
Oct. 1	22 4 10	0 11 34	27 14 50 N.
	22 40 11	0 11 14	27 14 55
	22 57 12	0 11 12	27 15 0
	23 29 54	0 11 7	27 15 10
	23 57 50	0 11 3	
	0 7 45	0 10 57	27 16 0
	0 25 40	0 10 54	27 16 10
2	21 39 7	0 4 22	27 42 5
	22 0 4	0 4 18	27 42 5
	22 29 56	0 4 9	27 42 10
	22 45 51	0 4 4	27 42 10
	22 59 46	0 4 0	27 42 15
	23 26 0	on an 8-mag. star.	
	23 30 0	just past.	
	23 45 36	0 3 50	27 42 15
	23 54 33	0 3 47	27 42 20
3	21 43 34	23 57 34	28 2 55
	22 24 22	23 57 22	28 3 10
	22 40 18	23 57 18	28 3 20
	22 57 12	23 57 12	28 3 25
	23 30 6	23 57 6	28 3 30
	23 45 2	23 57 2	28 3 30
4	22 22 12	23 50 12	28 22 15
	22 31 10	23 50 8	28 22 20
	22 50 8	23 50 6	28 22 20
	23 20 57	23 49 57	28 22 30
	23 49 50	23 49 50	28 22 40

Oct. 1 nucleus tolerably bright
2 ,, fainter.
3 ,, very faint. Moonlight
4 ,, barely visible. Moonlight.

On the 2nd, at $10^h\ 52^m\ 20^s$ G.M.T., the comet was on what I judged to be an 8-mag. star. The nucleus of the comet passed, I think, over the star, but it may perhaps have been a little north of it. When I thought the star and the nucleus were nearest I could not distinguish the comet at all, as the light of the star, small as it was, overpowered it; and the nucleus was some little distance, about its own apparent diameter, from the star before I could see it again.

The weather on the 5th October was very thick, and continued so for several days, thus preventing further observations.

Lat. 53° 28′ 24″ N.; Long. 0° 12′ 7″ W.

Waterloo, near Liverpool,
6th Dec. 1870.

Memoir by Hansen on the Transit of Venus.

The recently published Part V. of the ninth volume of the *Abh. der K. Sachs. Gesellschaft*, contains, pp. 457–552, an elaborate memoir by Dr. Hansen, "Bestimmung der Sonnenparallaxe durch Venusvorübergänge vor der Sonnenscheibe, mit besonderer Berücksichtigung des im Jahre 1874 eintreffenden Vorüberganges." Taking the mean distance of the Sun from the Earth to be a given number of units (for convenience it is put = 640), the unknown quantity upon which the solar parallax depends is the equatorial radius of the Earth, represented by ρ_0 and comparing, at a given instant of time during the transit, th angular distance of the centres of the Sun and *Venus* as see from the centre of the Earth with the angular distance as o served at a given position on the Earth's surface, we have f the determination of ρ_0 a quadratic equation

$$m^2 \rho_0^2 l^2 \cos^2 H - 2 m \rho_0 l S \cos H \cos (W' - \Sigma) + S^2 - u^2 = 0, \qquad (\S\ 15)$$

which is the fundamental equation. The accuracy of the d mination depends chiefly on the values of the two quant cos H and cos H cos θ_0, where H is the Sun's altitude, and the position-angle of *Venus* (or ∠ Z S V, if Z is the zenith c place of observation, S, V the centres of the Sun and respectively); viz. each of these quantities should be as la possible: cos H = 1 would, however, imply H = 0°, and count of refraction, &c., the observation would be impract it is, therefore, assumed that at the time of observation, t must have a certain altitude, say cos H = k, where k is as may be; the other condition then leads to the consi of the curves cos H cos $\theta_0 = k$, viz., k having any given v locus of the points on the Earth's surface for which this is satisfied is said to be an "isothenic curve;" and given isothenic curve the most advantageous position which cos $\theta_0 = \pm 1$; the locus of the points for whic mentioned condition is satisfied is said to be a "princip

curve" (Haupthöhen-curve). It is shown that the isothenic curves are small circles on the sphere. The memoir contains various numerical results in regard to the transit of 1874, and also two maps, a northern and a southern hemisphere, on the stereographic projection, exhibiting the isothenic and chief-altitude curves for the phases of ingress, egress, and nearest approach respectively, and also the eastern and western limit-curves of the transit. A conclusion arrived at is, that "By distance-measurements, at the instant of greatest phase, in favourable localities, the parallax can be obtained almost independent of errors in the longitude of the place of observation, or in the time: for such observations there is not any favourable position in the southern hemisphere—but in the northern hemisphere a wide field—in Siberia from about Tomsk to the Sea of Ochotsk, the region of the Amoor, the coasts of Mantchuria, the Japanese and part of the Kurile Islands, the peninsula of Corea." It is here, Dr. Hansen thinks, that the best results are to be obtained from the approaching transit.

There are three Appendices, the last of which contains formulæ for the application of photographic observations to the determination of the parallax. The formulæ require (besides the distance of the centres of the Sun and *Venus*) the determination of the position-angles of *Venus* at the two stations respectively. It is suggested that there should be at the focus of the instrument a wire in the plane of the declination circle, to be represented in the photograph, and thus to serve as a ground-line for the measurement of the position-angle, but the author is unable to judge as to the degree of accuracy with which the position-angle can be in this way determined.

As regards the geometrical theory it may be remarked that if a right cone intersects a sphere, the plane through the axis of the cone and the centre of the sphere (say the axial plane) meets the curve of intersection of the cone and sphere in two or four points which may be called vertices; moreover, we may, in the axial plane at right-angles to the line joining the centre of the sphere with the vertex of the cone, draw a diameter, the extremities of which are, say, the two points, K, K′. Now, considering the cone as variable (viz., in the astronomical problem the sphere is the Earth regarded as fixed in space and about its axis), the principal-altitude-curves are the locus of the vertices; and if with the variable centre K or K′ we describe on the sphere a circle of a constant given radius, the locus of the intersections of this circle with the intersection-curve of the cone and sphere is (or may be taken to be) one of the isothenic curves.

New Comet (IV. 1870).

Extracts of Letter from Dr. Winnecke to Mr. Hind.

Carlsruhe, 1870, *Nov.* 24.

Last night a small telescopic comet was discovered by me near *γ Virginis*; from five comparisons with Schjellerup 4610 its position is :—

M.T. Carlsruhe.

Nov. 23, $17^h\ 52^m\ 15^s$ R.A. $= 12^h\ 42^m\ 33^s\!\cdot\!36$ $\delta = -3°\ 29'\ 19''\!\cdot\!9$.

The daily motion in R.A. is about $+15^m$; in Decl. not sensible. The Comet is bright, round, and its diameter is about $2\frac{1}{2}'$.

Carlsruhe, 1870, *Nov.* 26.

From Carlsruhe, Nov. 23, 24, 25, I deduced the following orbit of the new comet :—

T = 1870, Dec. 19·836, Berlin.

$$\left.\begin{aligned}\pi &= 9°\ 25'\!\cdot\!8\\ \Omega &= 94\ \ 14\cdot9\end{aligned}\right\}\ \text{App. Eq.}$$

$$\iota = 30\ \ 14\cdot7$$

$$\log q = 9\cdot63244$$

Retrograde.

For middle observation $\Delta\lambda = +0'\!\cdot\!7$ $\Delta\beta = -0'\!\cdot\!1$.

New Comet.

Ephemeris from Dr. Winnecke's Elements by Mr. Hind.

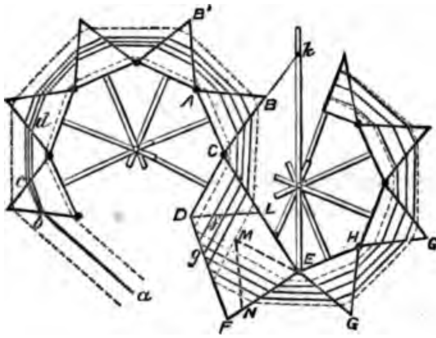
1870.	At Greenwich, midnight. Comet's R.A. h m	Comet's N.P.D. ° ′	Log. Δ	$\frac{1}{r^2\Delta^2}$
Dec. 1	15 32·8	96 8	9·6014	15·8
2	15 59·3	96 22		
3	16 24·3	96 34	9·6197	16·2
4	16 47·9	96 42		
5	17 9·5	96 49	9·6575	15·2
6	17 29·0	96 55		
7	17 46·4	97 0	9·7065	13·6
8	18 1·7	97 6		
9	18 15·0	97 12	9·7600	11·8
10	18 26·6	97 19		
11	18 36·6	97 28	9·8133	10·2
12	18 45·3	97 38		
13	18 52·7	97 50	9·8644	8·8
14	18 59·1	98 3		
15	19 4·5	98 18	9·9118	7·6

On a Contrivance for extending the principle of Mr. Browning's Automatic Spectroscope to a second battery of Prisms. By R. A. Proctor, B.A.

The principle of automatic adjustment devised by Mr. Browning has increased value the larger the number of prisms to which it is applied. So that if we require to use a second battery through which the light shall be carried after one reflection (and therefore with a deviation in a contrary direction to that taking place in the first battery), it becomes highly necessary that some contrivance should be adopted to secure automatic adjustment. This is yet more necessary if the light is to be sent back through both batteries.

The following plan, adopted after I had devised several others and rejected them on account of probable slight defects in their action, seems likely to be effective.

Suppose A B C to be the last prism of the first battery, G E H the first prism of the second battery, and C D F E an intermediate quadrilateral prism, so figured that the face, C E, belongs to the first battery—bearing the same relation to C B that A B bears to A B′—and that the face, E F, bears the same relation to E G that H G bears to H G′, while D F is inclined at an angle of forty-five degrees to the axis, *f g*, of the pencil of light, *a b c d*, passing parallel to C D. Then it is obvious that we only require that the imaginary prisms, C D L (shaped as A B C, &c.) and E N M, (shaped as G E H, &c.) should be kept in true and similar automatic adjustment with respect to the other prisms of the two



batteries. This is secured by having slots so attached to the prism, C F, as to be rectangular bisectors of C D and M E (C D L, M N E, being imaginary component prisms of the first and second batteries), while a slotted bar, E *k*, is caused to be always at right angles to O C by pivoting round E and slotting at *k*, where C *k* is a rod equal in length to the line C E.

The angles of C F are easily determined. C of course is equal to a base-angle of any prism of either battery. D is an angle of 135 degrees, E is a right-angle.

I forbear from giving a more detailed description at present, because Mr. Browning is constructing a spectroscope on this plan, and an opportunity will be afforded when he brings the instru-

ment before the Society, of discussing more at length the principles on which its action depends. In this instrument a dispersive power equal to that given by 19 or 21 equiangular prisms will be conveniently secured.

But it is easily seen that neither the automatic movement nor the optical conditions require the prism E G H to be attached directly to the angle E of the quadrilateral prism. By a separation here, it becomes possible to extend the second battery, so as to secure a dispersive power equal to that given by 23 or 25 prisms.

It may be well to note that the only points which are novel in the above arrangement (so far as I know) is the plan by which the light is carried from one battery into another without any loss of light (the refractions being both *effective*, and the reflection total); and the extension of the automatic method to include both batteries. The return of the rays after reflection is, of course, no new device, having been suggested many years since, and applied successfully by Mr. Browning four or five years ago. In this plan, as in the modification I have suggested on Mr. Browning's plan, the optical condition which minimum deviation is intended to fulfil is secured with mathematical exactitude (setting aside the point referred to in my paper on the subject in vol. xxx. of the *Notices*, No. 9). Mr. Grubb has devised a plan by which minimum deviation through compound prisms is automatically secured; but minimum deviation, *per se*, and the optical condition which minimum deviation through a single triangular prism fulfils, are different matters. We can secure the former in rays of all orders of refrangibility, in the case of a compound prism as readily as in the case of a simple prism; but the latter we cannot secure save for rays of a definite order of refrangibility.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXI. *January* 13, 1871. No. 3.

WILLIAM LASSELL, Esq., President, in the Chair.

Lord Lindsay, 47 Brook Street;
Rev. J. C. Jackson, Manor Terrace, Hackney;
S. Cottam, Esq., Higher Broughton, Manchester;
T. Ribton, Esq., 48 Woburn Square;
W. H. Mahony Christie, Esq., Royal Observatory, Greenwich;
Joseph Drew, Esq., Weymouth; and
Ankitam Venkata Nursinga Row, of Vizagapatam,

were balloted for and duly elected Fellows of the Society.

Report of Observations, &c., of the Total Eclipse of the Sun, taken at "Le Maria Luisa" Vineyard, Cadiz, December 21–22, 1870, by Lord Lindsay and his Party.

Lord Lindsay writes:—

My party consisted of Lieut. Alex. B. Brown, R.A., C. Becker, Esq., Henry Davis, Esq., and myself. I also had with me three engineers, Messrs. Scott, Rogers, and Winson, the latter of whom is my laboratory assistant. Mr. Scott's services were kindly supplied to me by Mr. Browning, to whom he is chief engineer.

We arrived at Cadiz on the 4th December, and though I experienced some difficulty in getting the instruments, &c., passed through the Custom House, we got to our station on the evening of the 7th December.

The station chosen was the "Maria Luisa" Vineyard, belonging to Mr. Charles Campbell, and which, in a most kind manner, he lent to me for the occasion. My very best thanks are due to him for all he did for us during our stay in Spain.

The instruments used for observation of the eclipse were,—

1st. A $12\frac{1}{4}$-inch silver-on-glass reflector, mounted equatorially and made for an observatory in Scotland, N. lat. 57° $30'$.

The complementary angle required by the change of the latitude to $36' 41''$, was obtained by a massive framework of wood made of four square pieces dovetailed and bolted through with $\frac{1}{2}$-inch irons and screwed up. The base plate of the instrument stood on this frame-work, and the final adjustments were got by means of screws in the base arranged for the purpose.

The position was roughly laid down by striking the lines of shadow, when the Sun had equal altitudes, and by bisecting the angle so obtained; however, I was unable to place the telescope correctly in the meridian for several days, owing to the bad weather.

This telescope stood in a house which I took out from England for it. One end was fitted up and used for a dark room for photographic purposes. The house was 20 feet long and 11 feet deep. Of this 6 feet by 11 was devoted to the dark room: the remainder to the telescope. It was covered all over with patent felt, and doors were made to open the entire front of that part in which the telescope stood. The roof also was made to cut and slide back to give a view 5° north of the zenith.

The second large instrument was a 6-inch equatoreal by Troughton and Simms, belonging to my observatory in Lancashire. To this instrument was attached a Star-spectroscope by Browning, kindly lent me for the occasion by Mr. David Gill, F.R.A.S. This instrument was fitted with a single prism of 60°, and of moderate low dispersive power. This telescope I intrusted to Lieut. Brown, whom I wished to examine the spectrum given by the corona.

There was also an altazimuth instrument of Troughton and Simms with vertical and horizontal circles $18''$ in diameter. This being a remarkably steady instrument, I used it as a transit-circle, &c.

I also mounted a $3\frac{1}{4}$-in. glass by Cooke and Sons, of York, on a wooden stand put up to the altitude of the pole.

We had very bad weather up to the time of the eclipse, in fact, the 21st was the only really fine day we had.

The morning of the 22nd was very cloudy till 8 o'clock, when it began to rain heavily, but this only lasted about thirty minutes, and then cleared up, and the Sun shone bright, though there were still heavy clouds passing at intervals over the disk. About 8 o'clock my party was augmented by the arrival of Mr. Reade, H.B.M. Consul-General of Cadiz, Mr. C. Campbell, H.B.M. Consul General of Puerto, Mr. Greaves, Navigating Lieutenant H.M.S. "Lee," Mr. Pitman of Puerto, Messrs. A. and E. Thuillier, and Señors Lassaletta and Gonzales.

Mr. Reade and Mr. Campbell and Señor Brogi undertook to assist Lieut. Brown at the spectroscope, while Mr. Pitman kept the time for me. Mr. Greaves measured the angular distance of the streamers of the corona and also the chief prominences

from the vertex of the Sun as seen in an inverting telescope. These angles were recorded at the time on a position-circle attached to the telescope. The Messrs. Thuillier and Señors Lassaletta and Gonzales made eye-sketches of the corona. These, when compared with the measurements of Mr. Greaves, show a good deal of correspondence *inter se*, though at first sight they might appear to differ.

The first contact was observed by Lieut. Brown with the 6-inch equatoreal, and also by myself with the 3-inch. This occurred at $22^h\ 50^m\ 3^s{\cdot}5$, G.M.T., or in local time, $22^h\ 24^m\ 59^s{\cdot}7$.

I secured several plates during the partial phase of the eclipse, but I was prevented from taking as many as I wished by the stop of my instantaneous slide giving way from the concussion, owing to the strength of the spring. Everything was ready as totality drew near, and the last minute was one of intense anxiety. The time observed of the commencement was $0^h\ 14^m\ 52^s{\cdot}50$, G.M.T., local time, $23^h\ 49^m\ 48^s{\cdot}7$.

During totality I had assisting me at the 12¼-inch equatoreal Mr. Scott, to watch through the finder that the telescope did not shift from its position while changing the plates. Mr. Rogers took the plates from the dumb-waiter, up the partition of the dark room, and handed each fresh one to me and put back the last exposed plate. Mr. Davis and Mr. Winson were in the dark room; the latter coated the plates and put them into the baths, while Mr. Davis received the exposed plates developed, and placed them in a fixing trough.

I had several times previously gone through the drill of totality, so that we all knew our work and about the number of plates we could run through in the time, and I had found that allowing rather short exposures, I could generally put twelve to fourteen plates through the camera in $2^m\ 10^s$. I did not, however calculate on getting as many as that, for fear of under-exposing them.

The last plate of totality was lost by my not pulling the slide open quick enough, as I missed the knob to catch it by, and when I had got it open totality ended.

This picture was developed, but not a sign was to be seen on it. In fact, as soon as I opened the slide I closed it again in hopes that I might have got a picture just as the Moon began to leave the Sun, but I must have miscalculated my exposure, as certainly if I had done this there must have been an impression on the film. I think, therefore, the exposure must have been over about $0^s{\cdot}5$ before the totality ceased.

The time was taken as the first glimpse of sunshine reappeared, and is in G.M.T., $0^h\ 17^m\ 0^s{\cdot}5$, local time, $11^h\ 1^m\ 19^s{\cdot}7$, which gives a duration for the total phase of $2^m\ 8^s$.

Immediately after totality I replaced the instantaneous slide which had been removed during totality; but I was not fated to get another picture, as while the camera was being altered, a dense rain-cloud came up rapidly and entirely obscured the Sun,

and although I made several attempts they were quite unsuccessful. In fact, we did not get a glimpse of the Sun again the whole day, as it set behind the same cloud.

I then went round and collected the various reports from the rest of my party. Four gentlemen had been successful in getting sketches of the corona and some of the prominences.

I will now pass to the general observations made before and during totality.

At 11.40, local time, the sky was of a purple tint, earth red-brown; everything green seemed intensified in colour.

At 11.45, a strange colour came over the landscape, dark and yet distinct, persons looked livid, cumulo-stratus below the Sun.

At 11.47, objects attained a yellow tint, which decreased in ntensity, and white with a red blue shade.

At 11.48, the clouds seemed to open out all round the Sun.

At the moment of totality the corona shot out on all sides of the Sun, and presented a most wonderful sight. The clouds in the north-east were purple; south-west, blue; north, grey.

Mr. A. Thuillier reports that, during totality, a flock of geese came towards him as if for protection, and the fowls all huddled up in a corner of the roosting-place, as if they were frightened.

I saw *Venus* plainly to the east of the Sun, and fancied that I saw another star, but cannot be sure.

I had requested all my party to notice particularly if the shadow of the Moon was to be seen advancing through the air, but none of them saw it.

The following is a Report by Lieut. Brown of his observations taken at Maria Luisa Observatory, between the 9th and 23rd of December, 1870, including the total eclipse of the Sun on the 22nd.

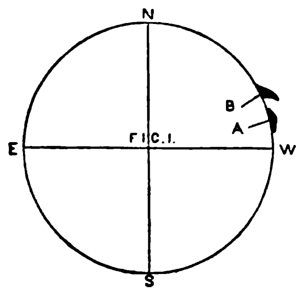
The general weather notes, also thermometric and barometric observations taken daily, I hand in as a separate form to accompany this paper.

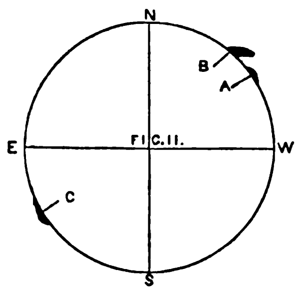
The instruments placed at my disposal were, 1st, a 6-inch equatoreal of 7-feet focus, moved by clock-work, made by Troughton and Simms. 2nd. A star-spectroscope, fitted with a single equilateral prism of very dense glass, very well adapted for spectroscopic analysis of the corona (or of the prominences during totality) made by Browning, of London. This instrument needed only to make it complete for its purpose, to have an automatic recording arrangement affixed thereto. I hardly realised the necessity for this till within a few days of the eclipse, so at once set to work to construct a recorder, and fitted it to the spectroscope, which, as will be hereafter seen, worked very well. Indeed, I know of no other mode of measuring the position of lines quickly that I would adopt during an eclipse. Any attempt to read off the position of the cross-wires in a graduated arc *several times* would be quite out of question, and any species of comparison references I should

equally reject as being confusing and untrustworthy, besides being very liable to derangement.

Every available observing day and night between the 10th and 22nd, the 6-inch equatoreal was called into use for various duties, transits, occultations, mapping of Sun-spots, spectroscopic observations (with the assistance of the spectroscope) on the spectrum of the Sun at different altitudes, of the Moon at different ages, and of *Jupiter*, and of one or two of the fixed stars; also of some of the solar prominences.

I made several successful, and I believe pretty accurate maps of the Sun's spots between the 11th and 22nd, one of which (B) shows fairly, and another (A) well, patches of faculæ. The four most successful maps were taken on the 13th, 18th, 21st, and 22nd, the last corrected up to the time of occultation of the spots by the Moon. These four accompany this Report, the others being less reliable, owing to unfavourable atmospheric influences, &c., I have thought unnecessary to preserve. The maps were made by the assistance of the 6-inch equatoreal, with powers of 31 and 65.





I had several opportunities of examining the prominences projecting from the edge of the disk of the Sun, but the most favourable occurred on the 20th and 21st instant. On the 20th I detected two prominences nearly joining, situated at A and B, Fig. 1, of which B was the largest and manifested a disturbed condition, it gave lines C; another (C′) rather more refrangible than C; one near D and F.

On the 21st I noticed B again, the action being more violent, and by examining it in different positions of the slit of the spectroscope I believe it to be tongue-shaped, as shown in Fig. 2, and estimated its height about 30,000 miles; it then gave lines as before, to which were added another near D and one less refrangible than F; they were of varied heights and continually altering in intensity.

The prominence A was smaller than the day before, but another prominence (C) was observed on the opposite limb of the

Sun more extensive, but not so high, giving line C (near), D (near), and F of different heights.

On the morning of the 22nd of December, the day of the total eclipse of the Sun, I carefully noted as usual the barometric, thermometric, wind, and weather readings at 9 A.M., which were

Barometer, 29·78, and falling.
Thermometer (in shade), 55° F.
Wind, W.; Force, 2½–3.
Weather, cloudy and overcast.

At $20^h\ 24^m$ (Greenwich time) the Sun shone out for a few minutes, the wind lightened and came round a little towards south, and we were cheered with the anticipation of a fair day. There arrived at this time Mr. Reade, Consul-General of Cadiz, Mr. Campbell, Consul of Puerto St. Maria, and Señor Giuseppe Brogi, who undertook to assist me in the spectroscopic and any time general observations I might require. These gentlemen were furnished with the necessary forms to fill up and instructions for observations, time-records, &c.

My first work, as the appearance of the Sun gave me opportunity, was carefully to map the spots, which were corrected up to the time of the the principal ones being occulted by the Moon. About a quarter of an hour before first contact a considerable change for the better was manifest in the weather, the patches of blue sky becoming larger and remaining longer free from clouds, though even then a faint haze was blowing across the Sun from time to time.

I carefully watched for first contact, which I noticed took place (Greenwich mean time) at $22^h\ 50^m\ 3^s{\cdot}50$ (Mr. Campbell taking time). From this time a more decided improvement in the state of the sky took place, and all my party were enabled to observe the Sun from time to time through my telescope.

At first contact I did not observe anything like spots of light on the Moon's limb or rugged semi-illuminated projections: it had the appearance of a circular disk with a smooth edge; the darkness of the Moon was equal to that of the surrounding sky; I could then notice no difference. Some time after, however, the jagged outline of the Moon was marked on the Sun. At the occultation of the spots the Moon appeared darker than any portion of the spots, which were then very clearly seen. After this occultation I fitted and adjusted my spectroscope, and carefully recorded the position of the solar lines C, D, E, b, F, G, h, both above and below the spaces left for corona and prominence observations by means of my automatic recording arrangement.

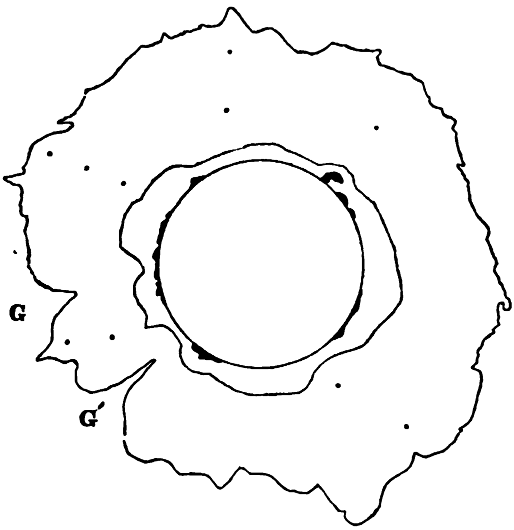
About ten minutes before totality Señor Brogi placed the cross-wires of my finder in the eastern limb of the Sun, and followed its motion, while I searched in vain for prominences which appeared *so* brightly shortly after; this was probably due to the haze which

more or less obscured the Sun at that time. This haze, however, blew over during the last ten seconds before totality, and the thin crescent of the Sun on my cross-wires enabled me to adjust my slit accurately at the last moment; the exceeding rapid disappearance of this luminous crescent in my spectrum (which disappeared from a decided measurable breadth), which now appeared almost dark, manifested to me that the long-expected moment had arrived; to my call time Mr. Campbell promptly replied, $0^h\ 14^m\ 52^s\cdot50$ (Greenwich time). I immediately directed my assistant to place the cross-wires of the finder on a portion of the corona about 8′ or 9′ from the obscured disk of the Sun, when I at once got a *continuous spectrum* about equal in intensity to that given by the Moon just before entering its third quarter, free from any lines bright or dark. I then tried to shut off the light *partially* by decreasing my aperture, my spectrum faded, still I got no lines. I opened it more than before, no appreciable alteration in the colour or brightness of my spectrum, but still no lines. I then worked with the slit at its original aperture, and directed my assistant to take some other places on the corona between 4½′ and 25′ (about the top of the corona) from the Sun's disk (which positions are marked in the sketch as black dots), the same result, a continuous spectrum free from lines. I searched in vain for the line 1474, and others whose positions I had before carefully determined; and, thanks to the kindness of Professor Young, I had particularly noted the celebrated American line in his high dispersive spectroscope a few days before at Xerez. My spectroscope gave no indication of any bright lines on that luminous portion that I believe to be the true corona, and which formed an irregular curve round the bright portion surrounding the chromosphere (as seen by sketch). This might be due to the fact of there being none to notice, or that the unfavourable atmospheric conditions did not enable me to detect any with my comparative low dispersive instrument. The parts of the corona I examined were from 4½′ to 25′ from the Moon's disk, the actual position being about 4½′, 7′, 12′, 20′, and 25′ from the Moon. I carefully avoided thus any chance of error in mixing up an achromatic chromosphere or prominences in my examination of the Corona.

During totality I wished to see if my assistant at the finder was following my directions, and so requesting him to stand on one side for a few seconds went myself to the finder, and saw the cross-wires on the part of the corona I wished to examine. For some ten or fifteen seconds I gazed at the dark orb surrounded by its glorious corona; and then I was struck by three prominences which appeared to me the largest of several on the disk, A, B, C, of which B was tongue-shaped. I then remembered my instructions to some of our party using telescopes and observing with the eye alone, for they were on the exact positions, and B also of the exact form I had predicted, from having noticed them in the spectroscope the day before. I was determined at once to

test their constitution, to see if they would give the same lines as I had before observed without an eclipse.

I have endeavoured to make a rough sketch of the picture* presented to me in the telescope and by the eye; one remarkable part of which was the difference I noticed the corona to have near the Sun, it seemed to me distinct from the rest of the corona, and to be irregular in outline, bulging out or extending much further from the Sun in some places, whereas in others it took the form of concentricity with the Sun; it had, moreover, the colour and appearance of pearl, with a bright phosphorescent tone about it, and was similar and dense throughout, whereas the rest of the corona had a faint violet colour, tinged in places with faint green and faint yellowish red, and decreasing in luminosity as its distance from the Sun increased; moreover it was jagged somewhat in outline and had gaps in it (as seen in sketch), and very faint striations might be seen in it; the inner portion, as I



have said, was quite different; and I should consider it an matic chromosphere surrounding the regular chromosphe separating it from the corona. I would venture to sugg name of Leucosphere for it, in contra-distinction both to mosphere and corona. I consider it to partake more of th acter of the former, the latter (Corona) I believe to be sola the variations in colour and faint streamers may be parti to the passage of light from less luminous portions thro atmosphere.

* The woodcut is merely an outline from a finished coloured dra Corona sent by Lieut. Brown.—*Ed.*

To return, however, to the prominences, many of which were bright red (and could easily be seen by the naked eye), while others were pink and several more or less tinged with violet at the upper edges. (I have endeavoured to sketch them in my drawing which in rough outline was made immediately after totality.)

I directed my cross-wires to be placed on the prominence B, and immediately got the line as follows :—

	Name of Line.	Prominence B. Angular Value.	Tongue-shaped. Reduced to Kirchhoff Scale.
	C	$109^\circ\ 38'$	695
(more refrangible than C)	C′	109 41	730
near	D	110 4⅓	1015
(less refrangible than E)	E	110 43	1470
	b	110 56¼	1635
	F	111 33⅓	2080
	h	113 20¾	3370

The heights of these lines were different, and violent action was manifested, especially in the lines C, F, *h* ; the heights I was unable to measure, but have entered as carefully as I could judge with the eye; the greatest height of the prominence seemed to be $2\frac{1}{4}'$, it was bent, and near top was less red in colour, tinged with bluish violet ; its estimated height was 55,000 miles.

The prominence A gave six lines ; the positions were not recorded, but I believe them to be at C, two nearer D, one near E (which seemed to be in a position midway between the E mentioned above and the true E), one either a little less refrangible than C or identical with it ; these lines were all shorter than those in B, one near E being the highest ; they underwent less change. I did not have time to mark on my recorder their position, being anxious to pass on to the examination of C on my scale.

Prominence C.

	Name of Line.		Angles.	Reduced to Kirchhoff Scale.
	C		$109^\circ\ 38'$	695
(near)	D		110 4⅓	1015
(near)	E	less refrangible	110 43¼	1475
	F		111 33⅓	2080
(near)	G	(probably G)	112 37½	2850

The lines C, E and G were the highest, E being the highest of the three ; then came G as a broken flickering line, part being seen bright near top and at bottom with variable brightness in

the intermediate part. There seemed to be a short line near *b*, and another short one more refrangible than G, which I could not mark as the dark disk of the Moon seemed to be rapidly covering the prominence. I devoted the rest of the time to a further examination of the corona with the same result as before. I also directed my cross-wires to be placed on the gap (marked G in figure), and then the spectrum gave no lines, but almost entirely faded away, even upon further opening the slit; the green and blue portions seemed to be comparatively brighter with respect to the red end than before.

As totality was nearly ended I directed Señor Brogi to take up a position on the advancing limb of the obscured Sun, when, after watching for three or four seconds, a thin crescent of light illuminated my spectrum, growing suddenly broad and bright, and showing the dark solar lines distinctly, giving me conclusive proof that totality was ended. I at once called time and was given $0^h\ 17^m\ 0^s{\cdot}50$, making the length of our observed totality just $2^m\ 8^s$.

With respect to the measurement of the lines in the prominences, I have endeavoured as accurately as possible to get the angular value, and from my marker believe I am correct to half a minute, but at any rate I cannot be ten divisions out on any line, taking the position of C on Kirchhoff's scale to be 695.

I will close this Report by adding the few remarks made by my assistants and handed to me.

Mr. Reade states, "At 12 minutes after noon darkness especially came on. Everything terrestrial had a leaden hue, while clouds assumed a purple colour; grass, however, was beautifully green. I saw a star or planet near the Sun (probably *Venus*)."

Mr. Campbell writes, "A cloudy day; as totality approached darkness rapidly came on. I had no need of the lamps for recording time and general observations by. I looked for a dark shadow to approach at time of totality, according to your directions, but saw none."

Senor Brogi says, "At the moment of totality, the Sun and Moon seemed to run together like one wheel running inside another in opposite directions; then immediately the corona appeared, and did not appear to fade away till after totality was ended; there was a haze blowing over the Sun part of the time."

I should add, amongst my own remarks, that, at $22^h\ 46^m$ G.M.T. a strong squall, with rain, lasting some five minutes, came on, and during totality the wind was measuring in force about 4; that darkness came on about ten minutes before totality, and the grass began to appear greener; that, during all this period, and indeed till totality, the sky and Moon appeared to me of equal darkness, but during totality the disk of the Moon ·arker than the sky considerably. This might have bee'

the effect partly of contrast, surrounded as the Moon was by the Leucosphere, and the corona encircling that. The edge of the corona most distant from the Moon seemed to fade in most places almost insensibly away, except where clouds gave it a more abrupt edge. They seemed to cut off some light at the edges. Doubtless, darkness during totality was less by reason of the diffused light scattered by the clouds. The amount of illumination was about equal to that given by the full Moon when a faint haze is blowing over, but of a different character.

We required no lamps for recording or reading by. The light was sufficient without. Some of the highest clouds at some 3° or more apparently from the Moon, seemed to be more illuminated by the light of the corona, Leucosphere, and prominences than those in other places.

At the moment of totality a feeling of loneliness and sadness seemed to come over one, and I could distinctly hear the subdued murmur of voices close at hand while the reappearance of the Sun seemed to bring a feeling of relief.

A few moments after totality was ended clouds blew over and the sky looked threatening. The Sun appeared for a few seconds only about a quarter of an hour after, and then disappeared for the rest of the day; heavy clouds blew up, and the wind rose, blowing at times gusts of 10 from the W.S.W. direction, with heavy rain. All this time, and in fact during the day, the barometer was falling: it read at A.M. 29·78, at 3 P.M. 29·61, and at 5 P.M. 29·55, still falling. Every effort to obtain last contact failed, and at 4 P.M. our flag, the British Ensign of the Blue, which had marked the Maria Luisa Observatory in the distance from the surrounding abodes, fell with its shattered staff. The elements seemed to say, We have up to the present held ourselves in check for your pleasure and for the success of your observations, and now that these are satisfactorily accomplished, we will have full play for our bottled-up energies.

Mr. Becker as to the Polarimeter observations reports as follows:—

The Polarimeter consisted of a Nicol prism in connection with a Savarts' band prism, attached to a box containing four glass slides, which can be turned so as to form different angles to the prisms.

The sky being covered nearly the whole morning, the preliminary observations of taking the sky polarization at certain intervals could not be taken, although the progress of observation could be well followed. As the time of totality approached, the sky brightened sufficiently in the neighbourhood of the Sun to see the totality with sufficient clearness. When totality was nearly complete, and directly after it was over, the polarimeter was directed towards the Sun, and not a trace of polarization was visible. At the time of totality the polarization appeared, the middle band being white, and was too intense to be thrown out

by turning the plates. By inclining the instrument to an angle of 23° to the west the bands disappeared. No difference could be observed between the Moon's disk and the corona. The polarimeter not being of a construction to localise a beam of light sufficiently.

General Remarks.

Protuberances were observed in three different places more in a cloudy form, as in the shape of horns at great prominences. Just before totality the whole sky had a leaden appearance. At the time of totality the blue patches in the sky appeared of a very deep purple colour, contrasting much with the rich brown appearance of the clouds.

The northern end of the crescent appeared like a string of pearls just before it was covered by the Moon.

On the Solar Eclipse of December 22, 1870, observed at Xerez in Spain. By R. Abbay, Esq.

The instrument with which my observations of the eclipse were made was a chemical spectroscope of two prisms of 45° each, kindly lent to me by Prof. C. A. Young, of Dartmouth, U.S. The diameters of the lenses of the collimator and telescope were 2¼-inches each, and the focal lengths about 17 inches. The slit was about ½-inch in length (nearly one-half of it being covered by the prism used for reflecting the light from the vacuum tube into the collimator) and was placed in a horizontal position. There was a horizontal and vertical motion of the whole instrument, so that the collimator could be kept on the Sun without difficulty for almost any length of time. The angle of aperture of the collimator was about 7°, so that when directed to the Sun the light passing through the prisms was composed of that proceeding from all points within an angular radius of 3½° from the Sun's centre, including, therefore, prominences, corona, and a portion of the sky surrounding them. A short time before totality began, I arranged the slit so that the D lines just appeared as a single thick line, this being the narrowest slit which it seemed safe to attempt to use, though I had determined to narrow the slit considerably if the bright lines appeared as bands on a continuous spectrum.

At 11.44, Xerez time, I noticed the B line extremely black. As totality approached the dark Fraunhofer lines slowly disappeared, leaving a dull spectrum, which also faded away immediately before three bright lines C D F (identified by means of the vacuum tube) made their appearance. These three lines came into view within two or three seconds after the shout announced that totality had begun, and they remained about 8 or 10 seconds.

C and D then disappeared, and two very sharp bright lines were seen, one coincident with the bright F line given by the vacuum tube, the other less refrangible than *b*. After some trouble I succeeded in placing the cross-wires very nearly on this bright line, and determined not to move the telescope during the rest of totality. No other lines appeared, although the C line of the vacuum tube was in the field on the one side and the F line on the other. I saw no continuous spectrum—the lines were bright on a dark ground. The F line was a little less bright than the other. On the reappearance of the dark lines after totality, I found that the cross-wires were on the vacant space between the lines 1464 and 1494 of Kirchhoff's scale. The measurement was as accurate as it was possible to obtain with the instrument used, and I cannot say with certainty that the bright line seen was absolutely coincident with the 1474 line. In order to give an idea of the dispersive power of the prisms, I may mention that after totality I tested the instrument by means of the light of the dull, heavy clouds which obscured everything, and found that I could not separate the D lines, but was able to obtain four thick lines like bands between E and *b*. I also saw 1464 and 1494 as single thick lines. By a rough calculation from the number of threads of the screw and the distance through which it was turned in bringing the jaws of the slit into contact, I came to the conclusion that it was from $\frac{1}{250}$-in. to $\frac{1}{300}$-in. wide. At the end of totality I noticed no reappearance of the bright lines C and D, nor do I remember at what moment the continuous spectrum came again into view. At about the middle of totality I looked up for a second or two at the corona; it appeared to the unaided eye to be distinctly and unevenly radiated. The light was of a pearly white, apparently of about the intensity of the full Moon, and extended to a distance of half or two-thirds of the diameter from the Sun. The shadows cast by certain parts of my instrument seemed as deep as those of a bright moonlight night, and the ivory handle of my commutator, which happened to be in shadow, was difficult to find.

The gradual disappearance of the Fraunhofer's lines as totality approached admits of two explanations. Either it was due to the general weakening of the light making it impossible to perceive these lines, or it was due to the superposition, on the spectrum with dark lines produced by the general light of the sky in the neighbourhood of the Sun, of a spectrum consisting mainly of bright lines proceeding from the extreme edge of the Sun's limb, this causing the Fraunhofer's lines to be more or less obliterated.

From the disappearance of the bright lines C and D some eight or ten seconds after the beginning of totality, I inferred that until the instant of their disappearance the chief part of the light passing through the prisms was due to the semicircle of the chromosphere, together with the prominences, visible chiefly on one limb. After this, and during the central period of totality, the main portion of the light was given by that part of the envelope

of the Sun which is above the red flames (whether chromosphere or corona). It is possible the green line, which was the stronger, may be due to some substance in the highest portion of this envelope; and the F line, which continued during the whole of totality, to moderately cool hydrogen somewhat nearer to the Sun's surface. It is clear that the same spectrum will be obtained, during the middle of totality, from light proceeding from points anywhere apparently in the neighbourhood of the Sun, and even from that reflected from clouds, as the whole atmosphere is then illuminated by light of this peculiar kind, so that the spectroscope does not appear to me capable of giving any conclusive evidence as to the extent of this luminous envelope.

Oxford, Jan. 13th, 1871.

Solar Eclipse, Dec. 22, 1870. By William Stainer, Navigating Sub.-Lieut. H.M. Gunboat Pigeon.

Went on shore to visit the astronomers, and was fortunate enough to observe the eclipse of the Sun during its totality with a $2\frac{1}{4}$-inch inverting telescope belonging to Mr. James Buckingham, C.E. I distinctly saw a rim of light around the dark body of the Sun, extending to about $\frac{1}{30}$th of the Sun's diameter from the edge. I also saw three permanent shoots of light extending about $\frac{1}{10}$th of the Sun's diameter from the Sun's edge; one on the N.E., one on the S.E., and one on the S.W., besides several smaller ones between, they were reddish, close to the edge, and yellowish towards the extremity. The diameter of the rays of light was about $\frac{1}{3}$rd of their length; the interval that elapsed during the visibility of the corona was from five to ten seconds. *Saturn* I also observed distinctly below the Sun at a distance of twice its diameter. The telescope through which I made the observations was the finder of Mr. James Buckingham's large 9-inch telescope for photographing the corona; the Sun being kept directly on the cross-wires of the telescope by clockwork.

The Spanish observers, including the Astronomer Royal of Madrid, who were about a mile distant, saw nothing whatever of the corona during the Sun's totality, which at this place was $2^m\ 9^s$.

Solar Eclipse, December 22, 1870, *observed at San Antonio, near Puerto de Sta. Maria.* By the Rev. S. J. Perry.

Being prevented by a course of lectures, and by pressing observatory work, from attending the January meeting of the

R. A. S., I think I shall best conform to the desires of the Fellows of the Society, by bringing under their notice a few short notes on some of the more salient points connected with our late observations of the total eclipse.

1. Form of the Corona. Approximately quadrilateral; outline not very well defined. Greatest in extent over the two sets of red prominences; in the north-west quadrant it was discernible to a distance of about seven-eighths of the lunar diameter. No streamers or curves, but only a glow of light fading off, as it receded from the Sun; this uniform appearance was, however, broken by four or five slight darker gaps radial to the limb. The streamers seen at Lord Lindsay's station, where the sky was clear, were perhaps too delicate in structure to be visible through our clouds of cirrus. Further evidence seems required to establish the fact, that even these streamers are due to our atmosphere.

2. Within the Corona, but surrounding the chromosphere, is a narrow band of silvery white, whose width is about one-tenth of the solar diameter. The intensity of the light appeared to be uniform throughout this band, whereas it faded off in the corona as it got further from the Sun.

3. Duration of the Corona. I did not see the corona before totality commenced, as I was wholly engaged with my spectroscope; but from notes taken for me by Lieut. P. H. Worgan, of H.M.S. "Lee," I find that the end of totality was observed at $12^h\ 17^m\ 12^s$, G.M.T., and the disappearance of the corona at $12^h\ 17^m\ 52^s$. This interval is, I think, too long by a few seconds, but it could scarcely have been less than 35^s. This phenomenon was very striking, easy of observation, and quite unexpected at the moment.

4. Nature of the Corona. My own spectroscopic observations are merely negative, no bright or dark lines, with possibly a faint glimmer; but these observations, continued on different parts of the corona during about two-thirds of totality, are of little weight, as the light of the corona, rendered exceedingly feeble by the intervening cirro-stratus, could not be expected to penetrate through my three compound prisms. The bright lines seen on the dark body of the Moon by Capt. Maclear, R.N., show that we must receive with great caution any observations of bright lines in the corona, which are coincident with those of the chromosphere, especially when the bright lines of the prominences are dispersed by intervening clouds and atmosphere.

These remarks refer merely to my own personal impressions. The position of the place of observation is

Lat.	$36^\circ\ 37'\ 13''$, N.
Long.	$24^m\ 45^s$, West of Greenwich.

Solar Eclipse, Dec. 22, 1870. By C. G. Talmage.

We arrived at Gibraltar on the evening of Tuesday, December 13, it being late, we did not land until the following morning; Capt. Parsons and myself took up our residence at the Club-House Hotel. Our first wish was to fix on a good site for our observing station, and with this object in view, we consulted several old inhabitants, and after going round the Rock twice, we determined on making an old tower, belonging to the Moorish castle, our post. At the top of the tower there is a solid square terrace, at an elevation of about 300 feet above the sea-level, we found that this position was much below the mist and cloud line, the higher parts of the Rock being subject to frequent enshroudings in mist and cloud.

Wednesday, the day before the eclipse, was beautifully clear with a north-west wind, which generally lasts three days at Gibraltar; we went to bed with great hopes of a fine day on the morrow, but the wind unfortunately changed to the south-west during the night; and at sunrise a heavy bank of clouds rose in the west, and gradually got higher, so that, by nine o'clock, the sky was quite covered.

I proceeded to the Moorish castle at $9^h\ 15^m$, leaving Capt. Parsons on the line wall, as there seemed no chance of one place being better than another, or rather all seemed equally bad, so thick and threatening were the clouds, and by this time every part of the Rock was cloudy, as we were informed by telegrams, which Capt. Parsons had arranged to have sent to him half-hourly.

I was not able to see the first contact, but had glimpses of the Sun soon after; rain then fell for a short time, and again the clouds were a little thinner, the clearest time being from 60″ to 5″ before totality, when the clouds suddenly heaped up and completely obscured the Sun.

The time of totality was evident from the darkness, which was greater than I had anticipated. As there was no chance of seeing *Saturn* I glanced round, over the landscape, which was very striking, a thrill of horror seemed to run through one. I saw the lights in the town below distinctly, and to my right (nearly we I saw the fires burning in the cork woods behind Algeciras, ab twelve miles distant. The noise made by the people on the (Mole was suddenly hushed during totality, and revived with creased vigour after. The clouds got thinner a little after tot and cleared off sufficiently to allow of my observing the t last contact.

Work done at the Kew Observatory.

(Communicated by Warren De La Rue.)

Pictures of the Solar Eclipse were obtained at the Kew Observatory by means of the Photoheliograph at the following times:—

Dec. 22, 1870.

	h m s
(1)	1 7 12·5
(2)	1 12 26·5
(3)	1 17 55
(4)	1 28 29·5
(5)	1 33 21·5

Of the above only Nos. 1, 2, and 5 are capable of being printed to advantage. Had the weather proved favourable a large number of photographs would have been procured.

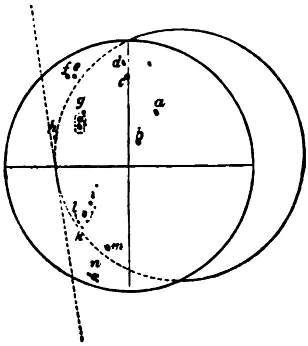
Solar Eclipse, December 22, 1870. By John Joynson, Esq.

First contact of limbs	$11^h\ 4^m\ 19^s{\cdot}1$ A.M. G.M.T.
Last contact of limbs	$1\ 35\ 55^s{\cdot}2$ P.M. „

Lens 3½-in. Inverting Eye-piece. Power 60.

The eclipse was seen under the most favourable conditions that could be desired at such a late period of the year. The sky was entirely free from cloud while the eclipse lasted; save at about $1^h\ 15^m$ P.M., when a few streaky clouds passed over the Sun; but which did not in any way interfere with the observations. The weather was exceedingly cold, the thermometer being about 36° even in the Sun's rays, and in the shade there was hard frost all day.

The observation of the first contact of limbs agrees to seven seconds with the calculated time for this place; and that for the last contact to two seconds. This no doubt arises from the fact that the last contact of limbs is always more easy to observe exactly than the first, as the limbs may be in actual contact before the time noted.



On the Sun's disk there were a considerable number of spots. The times of disappearance or of reappearance were taken for most of them; and in several cases they were observed both for disappearance and reappearance.

The position of the spots on the diagram must only be considered approximate; being merely eye estimates of their places just before the commencement of the eclipse. The times given are believed to be as exact as such observations will permit.

		D.	R.
		h m s	h m s
a	...	11 16 13	12 32 9
b	...	11 33 54	11 50 29
c	...	...	12 25 4
d	...	...	12 21 54
e	...	...	12 15 39
f	...	...	12 11 45
g	Spot, Upper	11 29 50	12 26 59
	Middle	11 31 4	12 29 19
	Lower	11 36 20	12 31 44
	Penumb.	11 30 19	
h	...	11 51 39	12 16 50
i	...	...	12 55 24
j	...	12 7 4	
k	...	12 13 48	
l	...	12 13 39	12 55 41
m	...	12 15 31	
n	...	12 24 19	

In reference to the spots the only remark seemed to be called for was that the two largest in the N.W. quadrant had very much the appearance of a dumb-bell; being connected together by a series of small black spots in a line between them, and having a wavy line enclosing the whole, as if the enclosed space *had* been one spot. These small black spots between the large ones visibly lessened in size while they were hidden by the Moon.

Very little was seen here of the eclipse of the Moon on the 6th inst., owing to dense cloud, which did not break until it was nearly over.

Waterloo, near Liverpool,
11th January, 1871.

The Solar Eclipse of December 22, 1870. By C. L. Prince, Esq.

The early morning of Dec. 22 was very cold, the sky completely overcast, and a few flakes of snow fell occasionally. The minimum temperature of the air had been 23°·2, and that of terrestrial radiation 18°. Wind N.E., and for several hours preceding the eclipse the horizontal movement of the air had been 18 miles an hour. The barometric reading from 9 A.M. till 2 P.M. remained almost stationary at 29·936. Soon after 9 o'clock the large masses of composite cloud began to break and disperse. At 10 o'clock the Sun shone brightly, and by half-past ten the southern portion of the sky became almost cloudless, and so continued till after the termination of the eclipse. I was, therefore, fortunate enough to have an uninterrupted view of the phenomenon as seen in this latitude. There were two conspicuous spots on the N.W. portion of the Sun, and many smaller ones scattered over its surface. The definition was better than might have been expected with such a brisk current of wind blowing from the N.E.

Shortly before the commencement of the eclipse some thin cirrostrati, which were drifting with a S.E. current, at a great elevation, exhibited very beautiful prismatic colours. At this time the temperature of the air at 4½ feet from the ground was 24°·6; at noon it was 24°·2; and at 2 P.M. 26°, which was the maximum for the day.

	L.M.T. h m s
The first contact occurred at	11 8 48
Preceding spot, of two, occulted, both Umbra and Penumbra	11 35 53
Following spot, of two, occulted	11 42 33
Emersion of preceding spot	0 37 58
,, following spot	0 40 40
Last contact	1 42 42
Magnitude of the Eclipse, as nearly as I could ascertain	0·820

At the time of greatest obscuration, a very *sensible diminution* of daylight was perceptible, and a peculiar yellowish golden light tinted all the trees and shrubs close at hand. The Moon appeared intensely black while passing over the Sun's disk, and it was interesting to observe how much darker the limb was than either the umbra or the actual nucleus of any spot. I particularly noticed that the penumbra of each was well defined to the instant of occultation. The inequalities both of the preceding and following limb of the Moon were very trifling as compared with those which I saw during the eclipse of March 1867. For the observation of this eclipse I employed my Tulley Equatoreal of 6·8 inches aperture and 12 feet focal length. Power 140. My Observatory is 24″ East of Greenwich.

Uckfield, January 3, 1871.

Solar Eclipse of December 22nd, 1870.
By J. B. Dancer, F.R.A.S.

Lat. 53° 28′ 21″·75; Long. $0^h\ 8^m\ 52^s$·47 W.

The eclipse of the Sun, on Thursday, the 22nd of December, was favourably observed here. Although a slight haze prevailed, all the details of the phenomenon were distinct, and tolerably well defined.

A number of spots were visible on the Sun's surface, two of which were of some magnitude. The nuclei of these spots were linked together by maculæ, and surrounded by a penumbra which extended to a considerable distance. Faculæ also were very numerous and distinct.

The times of contact taken by a chronometer carefully corrected to Greenwich mean time were as follows:—

	h m s
First contact of the Moon's Limb with the Sun ..	11 5 49
Contact of Moon's Limb with nucleus of the first large spot	11 31 36
With the nucleus of the second large spot	11 37 20
Last contact of Moon's Limb with the Sun	1 37 3

The temperature during the progress of the eclipse was taken at intervals by a mercurial thermometer with a black bulb in vacuo, exposed to the Sun at the height of 4 feet from the ground.

Time.	Temperature.	Time.	Temperature.
h m s	°	h m s	°
11 10 0	31·5	12 22 0	27·2
35 0	30·25	35 0	28·5
45 0	29·75	1 37 0	29·0
50 0	29·25		

By reference to the table of temperature it will be seen that at the time of the greatest phase the temperature was 4°·3 lower than at the commencement, and that the thermometer had risen 1°·8 at the time of last contact. A thin covering of snow was on the ground at the time.

I had an impression that the Moon's limb could be traced short distance from the Sun's disk at the upper and lower poin of contact. The black surface of the Moon, when projected (the Sun's disk, appeared very uniform in colour, and darker th any of the spots.

Immediately after the last contact I tried with powers and 180, to distinguish the Moon's disk, but did not su Light clouds were passing over the Sun and Moon at thir

The diminution in light was quite perceptible at the time of the greatest phase. Telescope employed with 4¼ inches object-glass, with powers of 80 and 180.

Old Manor House, Ardwick, Manchester.

Solar Eclipse of December 21–22, at Mr. Bishop's Observatory, Twickenham. By W. Plummer.

The following times were observed with a power of 70 on the 7-inch Equatoreal as high a one as could be advantageously employed.

	h m s	
First contact, Dec. 21,	23 6 28·87	Twickenham M.T.
Last contact, Dec. 22,	1 40 32·72	„ „

The longitude of the Observatory is $1^m\ 13^s \cdot 10$, west of Greenwich.

Observing with the comet-seeker, by Pistor and Martius, Berlin, Mr. Hind noted the first contact $0^s \cdot 6$ later.

Solar Eclipse of December 22, 1870, visible as a partial one at Armagh. By the Rev. T. R. Robinson, D.D.

It was observed with a 7″ achromatic by Cauchoix, power 75, and the times were recorded by a chronograph of Krille.

	Armagh M.T. h m s
Beginning of Eclipse	10 33 53·43
First limb on first of two large spots ..	10 58 24·70
First limb on a spot high in field	11 2 26·44
First limb on second of two large spots ..	11 4 10·55
First limb on second of small group ..	11 32 30·18
Second limb on second of two large spots ..	11 59 56·29
Second limb on upper spot	12 18 30·62
End of Eclipse	1 3 37·55

Observatory, Armagh.

On the Solar Eclipse of the 22nd of December, 1870. By Charles H. Weston, F.R.A.S., &c.

The day of the eclipse proved at my Observatory very unfavourable for critically examining its phenomena. The upper sky was marked by *cirri-strati*, resulting from a southerly current in high regions, while near the earth a strong north-easterly wind brought heavy *nimbi*, with the low temperature of 23° Fahr. in the shade. The consequence of the aerial strata thus differing abruptly both in specific gravity and moisture would necessarily be a bad definition for telescopic observations, and thus the appulse and advance of the Moon on the Sun's disc were disfigured with boiling undulations.

I had previously marked the groups of solar maculæ for further testing refrangibility at the lunar surface, and especially the behaviour of the large spots, both as to their vertical and horizontal contour during the passage of the line of darkness over them. But the great undulations rendered futile all such attempts and all endeavours to trace irregularity in the Moon's edge, or to watch the solar cusps. Amidst this general failure, however, I noticed one interesting apparition, viz., that the disk of the Moon overlapping the Sun was *not uniformly dark, but enlightened for some distance from its periphery coextensively with the arc of contact.*

As I wished to test the correctness of my vision, I begged my friend Captain De Blaquiere (who had kindly undertaken to register all meteorological details throughout the eclipse) to come to the telescope and give me his impressions, when I found my own opinion confirmed.*

I shall pass over the several accounts given in our *Monthly Notices* of former eclipses connected with a bright border adjacent, but *exterior* to the Moon's limb, and the mathematical discussions to which they led, because such would be foreign to my present subject, which concerns an enlightenment *within* the Moon's disk. I shall, therefore, refer first to the account of the annular eclipse of 1858 given by Mr. Stuart.† The observations on this occasion were made under somewhat similar circumstances to my own, and the writer states that "the light from the [Sun] was very distinctly seen *within* the edge, and slightly illumin[ated] the dark body of the Moon for a short distance," and that h[is im]pression at the time was that "the solar light was inflec[ted by a] rare lunar atmosphere however doubtful." The same ecl[ipse was] also watched by Sir John Herschel,‡ and by Messrs. D[e la Rue and] Breen,§ and although with a clouded and broken sky, ... *very favourable defining conditions;* and then the lun[ar]

* Telescope used on that day was the Newtonian Reflector, ... meter and 9 feet focal length.

† *Monthly Notices*, vol. xviii. pp. 193–4. ‡ Ibid.

not contain any partial illumination, expressly so in the account of Sir John Herschel and Mr. Breen, and impliedly so in that of Mr. Dawes.

I have also referred to a description of the Solar Eclipse of 1836 by Admiral Smyth,* from which it would also appear that no partial illumination was then seen on the Moon, and although it is remarked that at first the lunar limb was somewhat tremulous, it yet soon became steady and *the definition pre-eminently fine.*

The suggestion cautiously thrown out by Mr. Stuart as to whether this lunar illumination resulted from the frangibility of a lunar atmosphere was *per se* a very natural one, but when considered in connexion with contemporary observations it cannot be deemed valid. The united testimony on that occasion of Sir John Herschel and Messrs. Dawes and Breen under such favourable circumstances has set at rest the question of any visible refrangibility, and, therefore, the rationale of the illumination of the lunar surface must be sought for in some other cause.

The next step was to learn what other Astronomers had noticed during the last solar eclipse. Sir John Herschel very obligingly replied to my questions, and I have his permission to state that he did not see any particular light on the Moon and that it seemed to be uniformly and absolutely black, and that certainly the Moon looked darker than the solar spots, *i.e.* their *average* illumination for the low magnifier used did not allow a sight of the nuclei.

The case, therefore, seems to stand thus:—When the definition is really good no such particular illumination of the Moon's disk seems to have been remarked; but when the definition is bad, and the Moon's edge boiling, then a greater or less amount of such illumination seems to have been observable.

Would not the inference consequently be that this apparition must rather be the result of the conditions of the atmospheric medium through which the celestial bodies were viewed than connected with the bodies themselves? Or does it not arise from "one of those strictly ocular nervous phenomena not properly subjective, but sensational," alluded to by the Astronomer Royal in his very valuable paper on the lunar luminous band?†

Ensleigh Observatory, Lansdowne, near Bath,
31st December, 1870.

Note on Oudemann's Theory of the Coronal Radiations. By Richard A. Proctor, B.A. (Cambridge).

The papers read at the last meeting on the subject of the recent eclipse were so full of interest that it seemed desirable not

* *Celestial Cycle*, vol. i. pp. 140–2.
† *Monthly Notices*, vol. xxiv. p. 18.

to prolong a discussion which was raised respecting Oudemann's theory of the coronal beams. But as some of the results which the eclipse observers obtained would appear to be invalidated if certain points of Oudemann's theory were admitted, I venture briefly to note two objections of which the second at least seems decisive against those points.

Admitting with Oudemann the probability that the interplanetary spaces are occupied to distances from the Sun far exceeding the radius of the Earth's orbit, by matter capable of reflecting a certain proportion of light, it yet appears improbable that the whole quantity of such matter within a distance from the Earth equal to the radius of the Moon's orbit could reflect an appreciable quantity of light. It seems exceedingly unlikely that under such circumstances the sky towards the meridian at night would remain to all appearance dark, while yet, according to Oudemann's view, the sky towards the Moon's place during total eclipse would be appreciably illuminated. It is undoubtedly true, as pointed out by Professor Adams, that some substances (black cloth or velvet for instance) are rendered visible by rays falling obliquely, whereas rays falling square to their surface are absorbed. But certainly all the forms of non-luminous matter which we *know of* as tenanting the interplanetary spaces, would be better seen when placed as planets are when in opposition than as planets are when near inferior conjunction. I need not point out why this is, or that it is not so much a question of the reflective capacity of the particles themselves, as of the proportion of their illuminated surface which is turned towards the Earth. Certainly a group of minute meteors near the Moon's place when she is full would send us much more light than the same group near the Moon's place when she is new.

But, quite apart from this, there is an argument which appears to me to render Oudemann's reasoning altogether inadmissible. Assuming that the quantity of illuminated matter lying on *this* side of the Moon during total eclipse would *by itself* be appreciable, yet the much greater depth of the same matter lyin beyond the Moon (and as well placed for the oblique illuminati required) would be enormously greater, and would also be il minated far more brightly. If a depth of 250,000 miles, full of s matter as Oudemann's theory requires, could give a certain qu *a* of light, ten millions of miles next beyond and towards th direction would undoubtedly give more than the quantity light, and the remaining 80 millions of miles lying yet beyond, towards the neighbourhood of the Sun himself give a quantity of light which would even render this l quantity wholly inappreciable.

It is to be remembered that Oudemann's theory, or special part of it which relates to the coronal beams, w explain the supposed mobility of the beams, as seen b and that all the other American observers in 186 that the beams were stationary. And further,

noticed that a single observation of the fixity of coronal rays (as, for instance, Bruhn's observation in 1860) is more convincing than any number of observations of apparent motion. For, seen as the beams must needs be (if beyond our atmosphere) through a medium whose condition is probably very variable during a total eclipse, we can readily understand that their aspect should sometimes seem to vary, or even that they should appear and disappear under the observer's eyes. But the fixity of a coronal beam cannot so be explained away; and those who are familiar with the history of eclipse observations are aware that many of the positive observations of this sort are often unexceptionable.

Now the great V-shaped gap in the corona last December was visible at widely separated stations; it is clearly recognisable in a photograph taken by the American observers in Spain; and it remained unchanged during the whole of totality.* A positive observation like this, relating to an object extending to a great distance (nearly half a degree) from the Moon's limb seems altogether unmistakable in its import. When it is added that the bright inner corona was perceptibly depressed where this V-shaped gap existed, results of very great interest are suggested.

Do not the observations made last December strongly support the view urged a year ago by many (myself among the number) that the corona is a solar aurora? *If the action of the radial solar forces which generate this aurora be supposed only to be more energetic over the spot-zone, and specially over the regions where spots actually exist, we should not only have an explanation of the so often noticed trapezoidal form of the corona, but also a suggested explanation of the observed association between the Sun-spot period and terrestrial auroras.*

On the Nomenclature of Matter exterior to the Sun's Globe.

By Richard A. Proctor, B.A. (Cambridge).

I venture, while there is yet time, to urge the adoption of convenient and expressive names for the phenomena presented during solar eclipses.

The word *photosphere* serves a very useful purpose, and I do not know that any other could be devised which would be nearly so suitable. But to the names *chromosphere* and *leucosphere* there are grave objections.

In the first place, the relation between the prominences and the layer of coloured matter at a lower level is such as to render the

* Since this was written I have had the opportunity of examining the best of Mr. Brothers' photographs (taken at Syracuse). This view, No. 5 of his series, far surpassing all other pictures of the corona in interest and value, shows the V-shaped gap opposite the south-eastern quadrant in an unmistakable manner. Indeed the gap is the most striking feature in the photograph. This disposes of the question.

term *Sierra* employed by those who discovered the layer altogether more appropriate than such a word as Chromosphere. Secondly, the name Chromosphere implies that the coloured layer forms a spherical envelope, which the irregularity of its sufficiently well-defined outline shows not to be the case. Thirdly, the word is not properly formed, *Chromatosphere* being, I apprehend, the correct form.

The objections to the word *Leucosphere* are even greater.* Such a word could not possibly be employed in descriptive astronomy without explanatory notes. And further it can scarcely be considered appropriate. For λευκός means *white*, and σφαῖρα means sphere; but the inner corona, when seen under favourable conditions, has not appeared white, and certainly it is not spherical. Furthermore, grave doubts exist whether the implied distinction between the inner and outer parts of the corona is more than apparent. It may be added that in all other combinations of the kind—as *atmosphere*, *photosphere*, and so on—(*hemisphere* belongs to another class) the first word of the compound is a substantive. There seems a valid objection to a change of plan in this respect.

But the great objection against both *Leucosphere* and *Chromosphere* consists in the utter unfitness of either for the purposes of descriptive writing. In this respect they differ wholly from the word *Photosphere*, which refers to a relation not likely to enter into descriptive passages; but objects such as those for which the names Chromosphere and Leucosphere have been suggested require expressive names. I can see no reason why the fine word *Sierra* should not be restored to its place in our books of astronomy; nor why, if it shall appear that a real distinction exists between the brighter and fainter parts of the corona, the former should not be called (as already by Schellen and Secchi) the *corona* and the latter the *glory*. Or else Professor Airy's mode of describing them might well be adopted, and one called the *ring-formed corona*, the other the *radiated corona*.

A Solar Fog-bow. By R. C. Carrington.

On the morning of November 27, at five minutes to ten, I curious bow formed in a mist, which was just rising; the S shining brightly, and on the principle of our motto, "*Quicq tet notandum*," I took immediate note of it. It was the smal' I have seen, not more than twenty-five paces from wher and it measured thirty-five paces from bow to bow. yard at a pace, very nearly. The colour was white, li bow. But what was more remarkable was the appea

* It was, of course, only in jest that at the last Meeting ...nnd of the word to that of *lycosphere* (a wolf-sphere).

centre, in which I could see the reflection of my head, which moved as I moved. I remember having seen a centre before in a rainbow, which I witnessed near Cader Idris, in Wales, in July, 1847, in a splendid bow of two arches, but which I never mentioned before, having never heard of the like.

Note on the Change in the Colour of the Equatorial Belt of Jupiter. By John Browning, Esq.

Since the appearance of my last paper in the *Monthly Notices*, Professor Herschel has kindly written me a letter on the subject of the change in colour of *Jupiter*. Some brief extracts from this letter will, I think, be of interest. Professor Herschel says,—

"I see in a recent number of the *Monthly Notices* that you are raising very interesting questions about the appearance of *Jupiter's* belts, which may lead to very important results if it is found that the coloured and disturbed appearances of the belts are subject to periodical maxima and minima at about the same time as those of the spots on the Sun."

In a number of the *Student*, published just a year since, I wrote an article on the physical condition of *Jupiter*, from which I now give an extract:—

"Another suggestion may be made here,—Sunspots are known to be periodical, their maxima and minima being about eleven years apart. As Mr. Huggins some years ago (date unknown) observed the equatorial belt of *Jupiter* to be of the same ochreish yellow as it is now, there is good reason to suspect periodicity in this change. A period of maxima of sunspots is now approaching. Have these two phenomena any relationship to each other?"

The information I have so much desired to obtain, as to when a change in colour had previously been observed, and the exact appearance of the planet at the time, Professor Herschel has kindly furnished me with in his letter. I, therefore, again give Professor Herschel's words:—

"On a fine night in January, 1860, I turned Mr. Prichard's 6¼-inch Equatorial, by Cooke, for about half an hour on *Jupiter*. The planet was so well defined, and the details of the markings on the equatorial belt were so peculiar, that I made a sketch of them, noting at the same time the remarkable brown colour of the equatorial belt. One of the edges of the belt (I think the upper side in the instrument) was beaded or divided into egg-shaped masses, which must have been of brighter or lighter colour than the background of the belt, to have given them so much prominence."

On January 7, at 9 P.M., three days before I received Professor Herschel's letter, I made a careful coloured drawing of the

planet, and the description given by Professor Herschel of the appearance of the coloured belt in January, 1870, would apply exactly to the appearance of the belt in this drawing. I have brought the drawing for inspection, but it is so similar to the one which appears printed in colours in the last Number of the *Monthly Notices* that it will not be necessary to print this as well.

My principal object in communicating this Note is to request that any observers who have kept a record of the appearance of *Jupiter*, at a date previous to the year 1860, will look through their note-books, and if they can find a note of the equatorial belt appearing to be coloured previous to that date will communicate the same either to myself or to the Society.

Clapham, January 9th, 1871.

Ephemeris of the Satellites of Uranus. By A. Marth, Esq.

For 8h Greenwich Mean Time.

		Ariel.		Umbriel.		Titania.		Oberon.	
		Pos.	Dist.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.
		°	″	°	″	°	″	°	″
Jan.	11	236	12	113	17	118	28	49	39
	12	104	12	15	21	65	27	23	44
	13	324	14	300	17	22	33	1	47
	14	175	15	206	20	350	35	341	45
	15	24	14	127	18	314	30	317	41
	16	243	12	26	20	264	27	287	37
	17	111	12	314	18	215	31	253	36
	18	330	14	211	19	181	35	221	40
	19	179	15	140	19	148	32	197	45
	20	30	14	37	19	103	27	176	47
	21	251	12	326	20	51	29	155	45
	22	118	12	224	18	12	34	130	40
	23	325	15	151	20	340	34	98	36
	24	184	15	51	18	300	29	63	37
	25	35	14	336	20	248	27	34	42
	26	258	12	238	17	204	33	11	46
	27	124	13	161	21	171	35	350	47
	28	339	15	66	17	136	31	328	44
	29	188	15	346	21	87	27	302	38
	30	41	13	254	16	38	30	268	36
	31	266	12	171	21	2	35	235	38

		Ariel.		Umbriel.		Titania.		Oberon.	
		Pos.	Dist.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.
		°	″	°	″	°	″	°	″
Feb.	1	131	13	81	16	329	33	207	43
	2	344	15	356	21	286	27	185	46
	3	193	15	270	16	233	29	165	46
	4	47	13	181	21	194	34	142	42
	5	273	12	98	16	161	34	113	37
	6	136	13	5	21	123	29	79	36
	7	348	15	286	11	71	27	47	39
	8	198	15	190	21	26	32	21	44
	9	54	13	113	17	353	35	359	47
	10	280	12	16	21	318	31	339	45
	11	142	14	301	18	270	27	314	41
	12	353	15	201	20	220	30	284	37
	13	202	14	127	18	184	35	250	37
	14	61	12	26	20	151	33	219	41
	15	288	12	314	19	109	28	195	45
	16	147	14	212	19	56	28	174	46
	17	357	15	140	19	16	33	152	44
	18	208	14	38	19	343	34	127	39
	19	68	12	326	20	305	29	95	36
	20	295	12	225	18	254	27	61	37
	21	152	14	151	20	209	32	32	42
	22	2	14	52	17	175	35	9	46
	23	213	14	336	20	140	31	348	46
	24	75	12	239	17	93	27	326	43
	25	301	13	162	21	43	30	299	38
	26	157	15	67	17	6	34	266	36
	27	6	15	347	21	333	33	233	38
	28	219	13	354	16	291	28	206	43
Mar.	1	82	12	171	21	239	28	183	46
	2	308	13	83	16	198	33	162	45
	3	162	15	356	21	165	34	139	42
	4	11	15	271	16	128	30	110	37
	5	225	13	181	21	78	27	77	36
	6	90	12	99	16	31	31	45	39
	7	314	13	6	21	256	34	19	44
	8	166	15	287	17	322	31	358	46
	9	16	14	191	20	276	27	336	44
	10	232	12	114	17	226	29	312	40
	11	98	12	16	20	188	34	282	36

	Ariel.		Umbriel.		Titania.		Oberon.	
	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.
	°	″	°	″	°	″	°	″
Mar. 12	319	14	301	17	155	33	248	36
13	171	15	202	20	114	28	218	40
14	21	14	128	18	65	27	193	44
15	238	12	27	19	22	32	172	46
16	105	12	314	18	348	34	150	43
17	325	14	213	19	310	30	124	39
18	175	15	140	19	261	27	93	36
19	26	14	39	18	213	31	59	37
20	246	12	326	19	178	34	30	41
21	112	12	226	18	144	31	7	45
22	330	14	152	20	99	27	346	45
23	180	15	53	17	48	29	324	42
24	31	13	337	20	10	33	296	37
25	253	12	240	17	337	33	263	35
26	119	12	162	20	297	28	231	38
27	335	14	68	16	249	27	204	42
28	185	15	347	20	202	32	182	45
29	37	13	256	16	169	34	161	44
30	260	12	172	21	133	30	137	40
31	125	13	84	16	84	26	108	36
April 1	340	14	357	21	36	30	74	35
2	189	14	272	16	0	34	43	39
3	43	13	182	20	327	31	12	43
4	268	11	100	16	282	27	356	45
5	132	13	7	20	231	28	335	43
6	345	14	287	16	192	33	310	39
7	194	14	192	20	159	33	279	35
8	50	12	115	17	120	28	245	36
9	275	11	17	20	69	27	216	40
10	137	13	302	17	24	31	192	44
11	349	15	202	19	351	33	171	44
12	199	14	129	18	315	30	149	42
13	56	12	28	19	267	26	122	37
14	283	12	315	18	218	29	90	35
15	145	13	214	18	182	33	57	36
16	354	15	141	18	149	31	29	41
17	204	14	41	18	105	26	6	44
18	63	12	327	19	54	27	345	44
192	90	12	227	17	14	32	322	40
20	149	14	153	19	341	32	294	36

	Ariel. Pos.	Ariel. Dist.	Umbriel. Pos.	Umbriel. Dist.	Titania. Pos.	Titania. Dist.	Oberon. Pos.	Oberon. Dist.
	°	″	°	″	°	″	°	″
April 21	359	14	54	17	303	28	261	34
22	210	13	338	20	251	26	228	37
23	71	11	242	16	207	31	202	41
24	297	12	153	20	173	33	180	44
25	154	14	69	16	138	30	160	43
26	3	14	348	20	90	26	135	39
27	215	13	257	16	41	29	106	35
28	78	11	175	20	4	33	72	35
29	304	12	85	16	331	31	41	38
30	159	14	358	20	289	26	16	42
May 1	8	14	273	15	237	27	355	44

In the night of Jan. 8 *Uranus* passed close to the place in the heavens, where it appeared on the evening of Jan. 11, 1787, when the two bright satellites were discovered by Herschel. In the interval between the two nights *Oberon* has performed 2278 whole revolutions and *Titania* 3523.

Ferndene, Gateshead, Jan. 18th, 1871.

Summary of Sun-spot Observations made with the Kew Photo-Heliograph during the year 1870.

(*Communicated by Messrs. Warren De La Rue, B. Stewart, and B. Loewy.*)

Months.	Days of Observation.	Days without Spots.	Numbers given to the New Groups in the Catalogue of Sunspots.	Number of New Groups.
January	11	0	No. 1126 to No. 1142	17
February	12	0	,, 1143 ,, 1168	26
March	14	0	,, 1169 ,, 1199	31
April	22	0	,, 1200 ,, 1230	31
May	25	0	,, 1231 ,, 1270	40
June	19	0	,, 1271 ,, 1309	39
July	18	0	,, 1310 ,, 1345	36
August	25	0	,, 1346 ,, 1389	44
September	21	0	,, 1390 ,, 1420	31
October	18	0	,, 1421 ,, 1459	39
November	17	0	,, 1460 ,, 1491	32
December	11	0	,, 1492 ,, 1528	37
Total	213	0	No. 1126 to No. 1528	403

The year 1870 was characterised by an exuberance of solar energy which is without parallel since the beginning of systematic observations (*i.e.* since 1825). The number of observed groups far exceeds that of any previous year; and it appears also from a

cursory comparison with the maximum year's observations, as recorded by Hofrath Schwabe, that the magnitude of the different groups, as well as the average amount of spotted surface during any period of the year, is unprecedented. Since accurate measurements on the area covered by the spots are wanting for any time previous to the Kew Cycle, it is, of course, impossible to speak on this point with absolute certainty.

The latter half of the year shows an increase of no less than thirty-five groups as against the first half; it seems, consequently, not at all certain that the real maximum has been reached or passed.

A very remarkable feature of the groups observed during the year appears to be their extraordinary lifetime. Although the calculation of the heliographic positions is not yet completed, and therefore the most reliable basis for judging on the identity of groups on their return to the visible surface is still wanting, yet there can be no doubt from the observations, that an exceedingly large number of groups completed 3, 4, and even more revolutions, before finally collapsing. Whether this peculiarity in the behaviour of groups belongs to all maximum years,—whether the groups of minimum years are on the whole of a more ephemeral existence,—and further, in what manner the duration of any single group is connected with, or dependent on, its magnitude and the law of periodicity, are questions very forcibly suggested by the observations of the past year.

January 6th, 1871.

Further Notes on the Floor of Plato. By W. R. Birt, Esq.

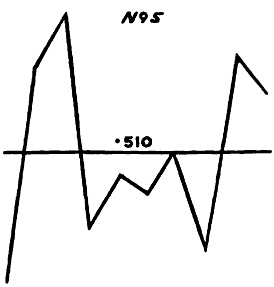
I have the honour to submit to the notice of the Society some further observations of the spots on the floor of *Plato*. In the *Monthly Notices* for April, 1870, vol. xxx., p. 160, will be found the normal degrees of visibility of the spots for twelve lunations ending March, 1870. The observations have been continued with the same care during the succeeding eight lunations, and now number 1594. The normal degrees of visibility have been determined for eighteen lunations; they are given in the following table:—

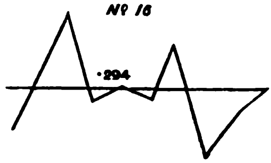
No.	Obs.	Vis.	No.	Obs.	Vis.
0	9	·046	9	43	·222
1	194	1·000	10	12	·062
2	9	·046	11	28	·144
3	174	·897	12	6	·031
4	173	·892	13	31	·160
5	99	·510	14	84	·433
6	43	·222	15	3	·015
7	22	·113	16	57	·294
8	3	·015	17	161	·830

No.	Obs.	Vis.	No.	Obs.	Vis.
18	14	·072	28	1	·005
19	29	·150	29	7	·036
20	9	·046	30	27	·139
21	5	·026	31	6	·031
22	34	·175	32	13	·067
23	9	·046	33	2	·010
24	11	·057	34	5	·026
25	28	·144	35	1	·005
26	1	·005	36	1	·005
27	2	·010			

In order to ascertain if the spots on the surface of *Plato* have been *equally* affected, which they should be upon the assumption of extraneous circumstances only producing changes in their appearance, such as solar illumination, alteration of position on the Moon's disk by libration and variations in the translucency and density of the Earth's atmosphere, the degrees of visibility for each pair of lunations between April 1869, and November, 1870, have been projected in curves. It is to the characters of these curves that I desire to solicit attention.

The general want of agreement which is found amongst the curves is at once conclusive that *all* the variations in visibility are not dependent upon the agencies above mentioned. On the other hand, the agreement subsisting between the curves of a few neighbouring spots points to some local agency, although in two instances spots at some distance from each other have manifested very similar phenomena, except in range; for example, spots No. 5

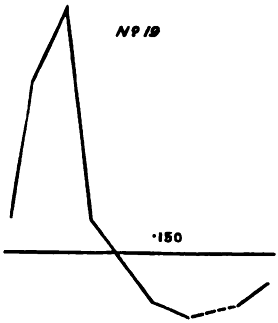


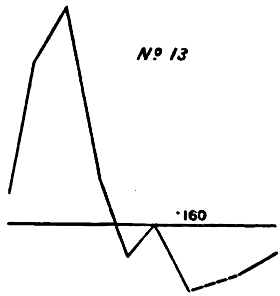


and 16; one not far from the south border, the other near the north border, have increased and decreased simultaneously during the eighteen lunations.

The curves are separable into three groups; the first distinguished by bold acuminated maxima, indicating a rapid increase of visibility and as rapid a decline, not unlike the maxima of some well-known variable stars which for a short time only are within the range of telescopic vision. Six spots, Nos. 19, 13, 22, 14, 5, and 16, on the western part of the floor, exhibited this rapid

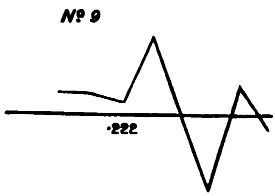
increase of visibility during August and September, 1869, in which spot No. 7 near the east border participated. The curves

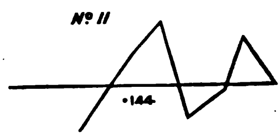




of Nos. 5, 16, 19, and 13 are given as specimens of those of the group.

The portion of the floor which may be regarded as the site of the second group is a band stretching from west to east on the southern half of *Plato*. Five spots, Nos. 9, 11, 18, 17, and 10 on





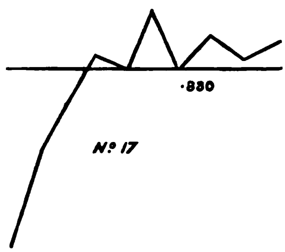
this band, with No. 29 near the north border, increased in visibility during February and March, 1870; the rise and fall being much less rapid than in August and September, 1869. The curves of Nos. 9 and 11 are given as specimens of this group.

In August and September, 1870, spots Nos. 18, 11, and 9, with the south-western spots 22, 14, and 5, and the eastern spots 6 and 7, increased in visibility, but not to the extent, except in No. 5, which characterised the increase at either of the previous epochs. These spots form the third group.

The above are not the only phenomena presented by the spots. In some cases we have nearly opposite curves, indicating opposite affections; for example, the curve of spot No. 19 on the north-west part of *Plato* is opposite in character to that of No. 17 on the south end. This is clearly apparent on comparing the curves. There appears also to be a general absence of agreement between the curves of those spots, as No. 17, which are known to be craterlets, as if the small white round spots differ from them specifically, or are affected by agencies that act more regularly. It is

to be remarked that the craterlets possess *high* degrees of visibility, *i.e.* they are much more frequently seen than the spots properly so called, which are mostly characterized by *low* degrees of visibility.

While these results appear to point to the operation of agencies the nature of which we are at present unacquainted with, they indicate the importance of continuing the observations, at least during another period of twenty lunations. It has already been



found that curves which in the earlier observations agreed in presenting similar maxima departed from a typical form in the latter, showing that the same spots were not under the same influences at the two periods.

It is not impossible that the prominent features of the curves may be explained upon the hypothesis of a present lunar activity. It is, however, desirable that observations should be multiplied, especially as there are strong objections to such a view, although the idea has been mooted during the last eighty years.

Solar Eclipse, June 22, 1870. By John Tebbutt, Jun., Esq.

The solar eclipse of last month was observed by me with the four-feet refractor and a power of about 30, as follows:—

	Windsor M.T. d h m s
Beginning ..	June 28 20 14 30
End	21 16 20

The Sun's limb boiled considerably, but both contacts, especially the last, were pretty well observed. Sun-spots were remarkably numerous.

Windsor, New South Wales,
1870, *July 11th.*

Occultation of δ^3 *Tauri.* By J. Joynson, Esq.

9th November, 1870.

		h m s
Disappearance	..	11 19 11·3 G.M.T.
Reappearance	..	12 16 50·7

The above observation was very satisfactory. The star disappeared at the bright and reappeared at the dark limb of the Moon; and the times above given agreed very closely with a calculation made for this place with the aid of Prof. Chevallier's method, which gave for the

		h m
Disappearance	..	11 19·0
Reappearance	..	12 16·5

Lat. 53° 28′ 24″ N.
Lon. 0 12 7 W.

Waterloo, near Liverpool,
6th Dec. 1870.

Notice of a presumed new Variable Star in the Constellation of Orion. By the Rev. T. W. Webb.

On December 25, 1869, while sweeping over a part of the constellation of *Orion* with a power of 65 on my 9-inch silvered speculum by With, I came across a red star, which I then estimated of the 10th magnitude, and considered less deep than R *Leonis*, but still of a conspicuous colour, not unlike R *Crateris*.

Dec. 27.—I have noted that it was one of a triple group, the others being smaller, about 11 mag. of the Bedford Catalogue, and situated at position-angles roughly estimated at 230° and 340°.

Jan. 26, 1870, on looking for this object with a power of 110, I was at once struck with its increased brightness: it seemed much more strongly contrasted with its two companions, and I thought it nearly of 9 mag.; the colour I considered not greatly inferior to that of Hind's crimson star.

Jan. 27.—I have noted that the triplet is in the middle of a large triangle of stars, the brightest of which, a white 9 m star, nearly precedes it; the other two, at the *nf* and *sf* ang being about 10 mag. The ruby star, approaching more nearl the brightness of the first than of the others, might be rated a' 9·3 mag. The accompanying rough eye-sketch may serve t an idea of the configuration.

Feb. 25.—I did not think it any larger than on Jan.

the colour struck me as very fine. I also noted two minute points north of it.

March 5.—If it had changed at all, it might be rather smaller, the colour remaining as before.

Thus far the observations seemed to indicate a probability of variable light, as in the case of so many ruddy stars; but I was not strongly impressed by it, as I thought my earlier estimates of its magnitude might not have been made with sufficient care.

On looking for it again, however, on Dec. 23, 1870, very nearly twelve months after its first discovery, with the same power of 65 which I had most frequently used on former occasions, I found that no doubt remained as to this point. I could only see a group of three minute stars of nearly the same magnitude, all too small to show such a colour as was vividly impressed on my recollection: I noted in fact no difference of tint among them: but had it existed, the object would have been far too inconspicuous to attract attention, as it had done, in casual sweeping. The eye-piece was now slightly dewed; but the estimate, being a comparative one, was unaffected by this circumstance. As far as I could recollect the situation of the red star, I should have expected to have found it the central one of the three; but on subsequently comparing my former observations without attending to the estimated position-angles, I was so far misled by the mention, on Feb. 25, of "two minute points north of it," and in some degree, perhaps, by an idea that it might still be the brightest of the group, that I concluded that it must be the *sp* of the three little stars.

Dec. 24.—There being a good deal of haze, I considered that this *sp* star could not exceed 11 mag., and I noted that it was but a trifle brighter than its companions.

Dec. 26.—In thin haze; the same star was found to be very little brighter than the other two, and decidedly much inferior to the two stars at the *nf* and *sf* angles of the including triangle, which, supposing it to have been the red star, it surpassed on Jan. 27.

At last, on Dec. 29, notwithstanding a little fog, I was able to discover the error into which I had fallen. I had a very satisfactory view of the group with 65, and the distinct ruby hue which I perceived, and confirmed with an achromatic ocular of 212, convinced me that not the brighter *sp* star of the triplet, but the central one, was the object formerly observed: thus giving incidentally somewhat more weight to the evidence of variation. I repeated my guesses at the position-angles, and found them as on Dec. 27, 1869. The magnitude of the red star I now estimated at 11, that of the *sp* star 10·5, that of the remaining *n* star of the group 11·2: the *p* star of including triangle being considered 8, the *sf* 9·3, and the *nf* 9·5. Two minuter points, *n* and *f*, may have been about 13 mag.

I have only one further observation to add, that of Jan. 9, 1871, when I had an impression that the red star was slightly on

the increase, as its brightness was little, if at all, inferior to that of its *sp* companion. I was then also enabled to recover the second minute point north of it, which I had not noticed since Feb. 25, 1870, and which probably may not exceed 14 mag.

I have little confidence in my own estimates of absolute magnitude, but the star in question is so very favourably situated for comparison, that I have no misgiving as to the relative values that I have assigned to it; and these appear to me to establish in a very satisfactory manner the existence of variable light; leaving for future examination its amount, its period, and its epochs. All that can at present be inferred is that one *maximum* may probably have occurred in the spring of 1870; and that if now, as I suspect, on the increase, it must have subsequently passed a *minimum* stage. The range of variation hitherto observed may be assumed as extending through upwards of 1·5 magnitude of Admiral Smyth's scale.

Through the kindness of Mr. Knott, to whom I communicated my earlier suspicions of variability, I am informed that this star appears in Schmidt's Berlin Chart, and is marked in the accompanying catalogue as of 9 mag. the white star *p* being rated 8 mag. On Jan. 5, 1870, Mr. Knott estimated the magnitude of the variable star 9·5.

I have at present no means of giving its place with accuracy; but it is easily found about $6^m\ 18^s$ in time west of 42 *Orionis*, and nearly on the parallel of a minute open pair, about 11 mag. 9′ or 10′ north of that star.

Minor Planet (110).

The following elements, calculated by Herr Oppenheim from observations, Bilk and Leipsig, April 20; Berlin, May 3 and 20; are given, *Ast. Nach.* No. 1805:—

1870, April 22·5 Berlin (?) M.T.

$$\begin{aligned}
M &= 193^\circ\ 18'\ 54''\!\cdot3\\
\omega &= 300\ \ 17\ \ 16\cdot5\\
\Omega &= 59\ \ 9\ \ 58\cdot6\\
\iota &= 5\ \ 52\ \ 11\cdot9\\
\varphi &= 4\ \ 38\ \ 15\cdot0\\
\mu &= 803''\!\cdot712\\
\log a &= 0\cdot429938
\end{aligned}$$

Minor Planet (111) *Ate*. Discovered 14th August by Dr. C. H. F. Peters.

The following elements, calculated by Dr. C. H. F. Peters, Clinton, N. Y. are given, *Ast. Nach.* No. 1822.

1870, Sept. 0·0, Berlin M.T.

$$\begin{array}{lcll}
M_0 & = & 208^\circ\ 17'\ 21''\!\cdot\!0 & \\
\varpi & = & 122\ 53\ \ 7\cdot3 & \rule{0pt}{0pt}\left.\right\} \\
\Omega & = & 306\ 26\ 28\cdot4 & \text{Mean Equinox } 1870\cdot0 \\
\iota & = & 5\ \ 1\ 21\cdot4 & \\
\varphi & = & 5\ 49\ 10\cdot6 & \\
\mu & = & 858''\cdot392 & \\
\log a & = & 0\cdot4108808 &
\end{array}$$

Comet II. 1870 (*Coggia's*).

The following system of elements, calculated from twenty-four observations up to the end of October, by Dr. Thiele, are given, *Ast. Nach.* No. 1826:—

$$\begin{array}{lcll}
T & = & \text{Sept. } 2\cdot18382 \text{ G.M.T.} & \\
\varpi & = & 7^\circ\ 52'\ 51''\!\cdot\!0 & \\
\Omega & = & 12\ 56\ 20\cdot3 & \text{Mean Equinox } 1870\cdot0 \\
\iota & = & 99\ 20\ 34\cdot2 & \\
\log q & = & 0\cdot259275 &
\end{array}$$

CONTENTS.

Printed by STRANGEWAYS and WALDEN, Castle St. Leicester Sq. and Published at the Apartments of the Society, Feb. 9, 1871.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXI. *February* 10, 1871. No. 4.

William Lassell, Esq., President, in the Chair.

T. G. E. Elger, Esq., Bedford; and
H. C. Russell, Esq., Superintendent of the Sydney Observatory,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Fifty-first General Meeting of the Society.

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
December 31, 1869	187	311	11	3	512	45	557
Since elected ...	+7	+18	...	...	...	...	...
Deceased	−4	−6	−1	..	...	−1	...
Removals	+2	−2	...	...	...	...	...
Resigned	...	−8	...	...	...	...	...
Names removed for non-payment of arrears, &c. ...	...	−8	...	...	...	...	...
Dec. 31, 1870 ...	192	305	10	3	510	44	554

Mr. Whitbread's Account as Treasurer of the Roy

RECEIPTS.

	£ s. d.	£ s. d
Balance of last year's account		102 17 6
By Dividend on £3400 Consols	49 18 9	
By ditto on £5000 New 3 per Cents ...	73 8 9	
By ditto on £3400 Consols	50 3 0	
By ditto on £5000 New 3 per Cents ...	73 15 0	
		247 5 6
On account of arrears of contributions ...	179 11 0	
186 annual contributions	390 12 0	
24 admission-fees	50 8 0	
17 first years' contributions	28 7 0	
		648 18 0
9 compositions		189 0 c
Sale of publications:—		
At the Rooms of the Society	41 3 0	
By Messrs. Williams and Norgate, Publishers	37 17 4	
		79 0 4
		£1267 1

tronomical Society, from January 1 *to December* 31, 1870.

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Editor of Publications	60	0	0			
Assistant Secretary	130	0	0			
Commission on Collecting 690*l.* 1*s.*	34	10	0			
				224	10	0
Taxes:—						
Land and Assessed	6	11	0			
Income	2	1	8			
Poor Rate	5	12	6			
Other Parish Rates	5	12	6			
				19	17	8
Bills:—						
Strangeways and Walden, printers ...	510	4	10			
Rumfitt, bookbinder	22	6	8			
Malby and Co., engravers	11	18	0			
W. Metcalf ditto	29	14	9			
Troughton and Simms	7	0	0			
Insurance	7	15	6			
Dr. Brünnow	30	0	0			
				618	19	9
Miscellaneous items:—						
House expenses	23	17	10			
Postages	40	14	8			
Books and parcels	4	17	1			
Expenses of evening meetings	13	13	0			
Waiters attending meetings	3	17	0			
Coals and wood	12	0	0			
Gas	6	9	8			
Repairs	2	12	6			
Sundries	9	10	9			
				117	12	6
				980	19	11
Lee Fund	5	0	0			
Turnor Fund	1	19	3			
Mrs. Jackson Gwilt's annuity, 1 year ...	8	16	0			
				15	15	3
				996	15	2
nker, New Cheque Book				0	5	0
				997	0	2
Balance at Banker's				270	1	2
				£1267	1	4

Examined and found correct, Jan. 27, 1871,

(Signed) H. PERIGAL, F. C. PENROSE, W. B. GIBBS, } *Auditors.*

Assets and Present Property of the Society, January 1, 1871:—

	£	s.	d.	£	s.	d.
Balance at Banker's	...	...	...	270	1	2
1 Contribution of 9 years' standing ...	18	18	0			
3 " 7 "	44	2	0			
7 " 6 "	88	4	0			
5 " 5 "	52	10	0			
14 " 4 "	117	12	0			
13 " 3 "	81	18	0			
19 " 2 "	79	16	0			
29 " 1 "	60	18	0			
Balance of an Account...	4	14	0			
				548	12	0
Due for Publications				2	7	0

£5000 New 3 Per Cents (including Mrs. Jackson's Gift, £300).

£3400 Consols, including the Lee Fund (£100) and Turnor Fund (£500).

Unsold Publications of the Society.

Various astronomical instruments, books, prints, &c.

	£	s.	d.
Balance of Turnor Fund (included in Treasurer's Account)	152	12	9

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	12	XIII.	199	XXVII.	456
I. Part 2	52	XIV.	392	XXVIII.	416
II. Part 1	70	XV.	174	XXIX.	448
II. Part 2	34	XVI.	200	XXX.	297
III. Part 1	86	XVII.	183	XXXI.	173
III. Part 2	105	XVIII.	177	XXXII.	204
IV. Part 1	103	XIX.	184	XXXIII.	208
IV. Part 2	114	XX.	180	XXXIV.	196
V.	126	XXI. Part 1	216	XXXV.	169
VI.	152	XXI. Part 2	100	XXXVI. (with M. N.)	255
VII.	176	XXI. (together).	91	XXXVI. (without)	47
VIII.	162	XXII.	182		
IX.	165	XXIII.	177	XXXVII. Part 1	518
X.	175	XXIV.	183	XXXVII. Part 2	476
XI.	185	XXV.	196	XXXVIII.	494
XII.	190	XXVI.	201		

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle,
The *Baker* universal equatoreal.
The *Reade* transit.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatorial stand and clock movement.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatorial stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{1}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{1}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and 2¼-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.

33. 2½-inch circular level horizon, by Dollond.

34. Artificial horizon roof and trough.

35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.

36. A pentagraph.

37. A noddy.

38. A small Galilean telescope, with the object lens of rock-crystal.

39. Six levels, various.

40. 18-inch celestial globe.

41. Varley stand for telescope.

42. Thermometer.

43. Telescope, with the object-glass of rock crystal.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Fuller* theodolite, to the Director of the Sydney Observatory.

The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument, No. 1, to Mr. Lassell.
Ditto ditto No. 2, to Mr. Huggins.
Ditto ditto No. 3, to Mr. Brothers for the Eclipse Expedition.
Ditto ditto No. 4, to Rev. C. Lowndes.
Ditto ditto No. 5, to Mr. Birt.
Ditto ditto No. 6, to Rev. J. Cape.
Ditto ditto No. 8, to Rev. C. Pritchard.
Ditto ditto No. 9, to the Director of the Sydney Observatory.
Ditto ditto No. 41, to Rev. C. Pritchard.
Ditto ditto No. 43, to Mr. Huggins.
The 6-inch circular protractor, to Mr. Birt.

PRINTED TRANSACTIONS OF THE SOCIETY.

Memoirs.

Part II. of Volume XXXVII., and Volume XXXVIII. of the *Memoirs* have been published since the last Annual Report. They contain the following papers, all of which are printed *in extenso.*

Volume XXXVII., Part II.:—

1. "On a Determination of the Direction of the Meridian with a Russian Diagonal Transit Instrument." By Capt. A. R. Clarke, R.E., F.R.S.

The instrument with which these observations were made was constructed for the Royal Observatory, Greenwich, by M. Brauer, of the Imperial Observatory, at Pulkowa, in such a manner that the rays of light, after passing through the object-glass, are bent at right-angles by a prism placed in the central tube, and then pass out at one of the pivots. The great advantage derived from an instrument of this construction is that the transit of an object at any altitude can be made without altering the position of the observer's head. This Russian transit was used, under the direction of Capt. Clarke, at Findlay Seat, a mountain in Elginshire, from October 13 to 31, 1868, for the special purpose of determining the true direction of the Meridian. The detailed results of the observations are given in the paper, and they give strong evidence in favour of the stability and excellence of the instrument.

2. "A Determination of the Constant of Nutation from the Observations in N.P.D. of *Polaris*, *Cephei* 51, and δ *Ursæ Minoris*, made with the Transit-circle of the Royal Observatory, Greenwich, 1851–65." With an Addendum containing "A De-

termination of the Constant of Nutation from the Observations of *Polaris* in Right Ascension." By E. J. Stone, Esq., M.A., F.R.S.

In this investigation Mr. Stone has availed himself of the results of all the observations of the above Polar stars in N.P.D. and of the deduced Right Ascensions of *Polaris* only, which appear in the "Ledgers" of the *Greenwich Observations* from 1851 to 1865. Special care has been taken to make the corrections for division-errors, astronomical flexure, and zenith points in the different years under one uniform system. The mean value of the constant as determined by Mr. Stone from the four series of observations is $9''\cdot134$, or $0''\cdot090$ less than Professor Peters' value published in *Numerus Constans Nutationis*. Tables containing the details of the computations are appended to the Paper.

Volume XXXVIII.:—

1. "Seventh Catalogue of Double Stars, observed at Slough, in the years 1823–28 inclusive, with the 20-feet Reflector; 84 of which have not been previously described." By Sir J. F. W. Herschel, Bart., F.R.S.

This Catalogue contains a number of double stars which were observed by Sir John Herschel in the course of his earlier stellar sweeps at Slough, and which, for various reasons, were omitted in preceding catalogues. Many of the estimated angles of position, Sir John Herschel considers, possess a considerable historical value, inasmuch as in many instances they were made at a date anterior to any recorded measurements, while others are the only existing records of position. This Catalogue, therefore, forms a valuable addendum to the previous Double-Star Catalogues of that accomplished observer.

2. "On the Determination of the Orbit of a Planet from Three Observations." By Professor Cayley, F.R.S.

This valuable mathematical paper is drawn up by Professor Cayley in considerable detail, and occupies the principal portion of the volume. The first sections are devoted to considerations on the general theory of the problem, followed by formulæ and deductions, illustrated by five lithographic plates.

Monthly Notices.

In addition to the preceding Papers, several others of equal interest are printed in full, or in abstract, in the *Monthly Notices*. Some of these are made the subject of special remark in another section of the Report.

General Index to the Monthly Notices.

In consequence of a frequently-expressed want of a means of more easy reference to the Papers and reports in the volumes of the *Monthly Notices*, the Council requested the Assistant-Secretary, Mr. Williams, to prepare a General Index of Volumes

I.–XXIX., which included the whole series at the time of going to press. The work of compilation was a laborious one, for it consisted not only of arranging the annual indexes into one index, but also of making a considerable number of additional entries both of names and subjects. The Index was placed in the hands of the Fellows in November last, and its value has been acknowledged by those who have had occasion to refer to it.

OBITUARY.

The Society has to regret the loss by death of the following Fellows and Associate:—

Fellows:—Prof. E. W. Brayley.
Mr. C. Mason.
Admiral Manners.
Mr. H. Boys.
Sir F. Pollock.
Rev. Dr. Gwatkin.
Mr. G. R. Smalley.
Mr. J. G. Perry.
Mr. C. D. Archibald.
Mr. C. Frodsham.
Lieut.-Gen. Sir W. T. Denison, K.C.B.
Mr. H. Barrow.

Associate:—Prof. H. Selander.

RUSSELL HENRY MANNERS was born in London on the 31st of January, 1800. He was the only child of the late Mr. Russell Manners, M.P. Having in early life evinced a desire for the naval profession, he was placed at the Royal Naval College on the 6th of May, 1813, where he remained until he completed the course of instruction which was to qualify him for his future career. On the 6th of March, 1816, he embarked as a volunteer on board the Minden, 74, Captain Paterson, in which, after assisting at the bombardment of Algiers, he proceeded to the East Indies. During his residence in India he served under the flag of Sir Richard King. On the 1st of July, 1818, he was nominated midshipman to the Orlando, 36, commanded by Captain John Clavell, with whom, in 1819, he returned to England in the Malabar, 74. After an intermediate employment on the Channel and West India stations in the Spartan and Pyramus frigates, under Captains William Furlong Wise and Francis Newcombe, he became, on the 29th of July, 1822, acting Lieutenant of the Tyne, 26, Captain John Edward Walcot, to which ship the Admiralty confirmed him on the 19th October following. In May, 1823, he rejoined the Pyramus, still commanded by Captain Newcombe, under whom he continued until he obtained his promotion on the 16th of August, 1825. His last appointment was on the 21st of October, 1827, when the command

of the Britomart, 10, was assigned to him. The Britomart was first employed in the Channel service under the order of the Earl of Northesk, Commander-in-Chief, at Plymouth. She accompanied the squadron of ships escorting Don Miguel to Lisbon in the early part of 1828. In consequence of the revolution that followed in Portugal on Don Miguel declaring himself absolute, the Britomart was stationed off Oporto to watch the British interests there. The Constitutional party, failing to restore the Constitution against the usurped position of Don Miguel, the British Government withdrew her Minister from Lisbon, leaving the British interests in the hands of the Consul only, and Captain Manners was selected to be in readiness to support him in case of need, by keeping in sight of signals from Lisbon as long as the safety of the vessel permitted, but not to anchor within any Portuguese port unless absolutely necessary. This involved a long and vigilant cruising off and on the coast for about eight months, and through the whole of the winter. The only place communicated with during that time was Gibraltar, and then only to receive a supply of provisions and water from the dockyard. The yellow fever unfortunately breaking out at Gibraltar just before going there for this object, no communication could be had with the town, and the stay was confined to from twenty-four to forty-eight hours. The zeal and ability with which this service was carried out by Captain Manners, as witnessed by Sir George Sartorius, then in command of the Portuguese Constitutional Squadron, and under whose orders in some degree the Britomart was placed, led to Captain Manners receiving his Post-rank on the 4th of March, 1829. He retired from active service in March, 1849, became Rear-Admiral in July, 1855, Vice-Admiral in 1862, and Admiral in September, 1865. In 1834 he married Louisa Jane, daughter of the Count de Noé, Peer of France.

From the time he obtained his Post-rank to the time of his death, the subject of our memoir devoted much of his attention to scientific pursuits. He was elected a member of the Royal Astronomical Society in 1836. At a very early period he took an active interest in its administration, and after being on the Council for some time, was elected one of the Honorary Secretaries in February 1848, an office which he filled until 1858, when he accepted that of Foreign Secretary. This was a post for which his knowledge of foreign languages and his position in society peculiarly fitted him, and during his tenure of office he formed by active correspondence a connecting link between English and foreign Astronomers. He was much esteemed on the Continent and in America. In connection with this circumstance it may be remembered that one of the Presidents of the Society, in asking Admiral Manners to transmit one of the Society's Medals to a foreign recipient, deemed it just to preface his remarks with the following well-deserved compliment:—

"Admiral Manners,—It has been my good fortune to visit the majority of European observatories, and to make the acquaintance

of their Directors and other gentlemen connected with them, and it has in consequence become known to me how high in their esteem our Foreign Secretary stands. Your urbanity and promptitude in carrying out our foreign business has indeed become proverbial."

Admiral Manners was, on more than one occasion, asked to accept the chair of President, which, after some hesitation, he consented to do, and he was elected to that position in 1868. None of his predecessors was more highly esteemed by the Fellows of the Society, and no one filled the Chair more admirably than he did. His mathematical attainments were considerable, more so than one might have inferred from his quiet demeanour. He was well versed in the astronomical literature of the day, and took a deep interest in the progress of astronomical science, both in England and on the Continent; and his active influence was always available for the promotion of any object connected with it.

On presenting the Gold Medal of the Society to Mr. Stone, First Assistant of the Royal Observatory, Greenwich, Admiral Manners delivered a most exhaustive summary of that astronomer's labours, and evinced a complete knowledge of the history of the Solar Parallax, for the investigation of which the Medal was mainly awarded. Illness overtook him before he could complete his second year of office, and he was compelled to forego the gratification of delivering the Address to M. Delaunay for his researches on the Lunar Theory; but he made it a point of duty and pleasure to receive M. Delaunay at his house, and although he was compelled to delegate to the friendly hand of Professor Adams the drawing up of the Address, yet he read and approved of what was written before it was delivered. As the season advanced he gradually grew weaker, and finally expired on the 9th of May, 1870. He was supported in his last moments by a sincere and profound belief in the truths of the Christian religion. Mrs. Manners survives him; also two sons and a daughter.

Admiral Manners was distinguished by an affability and kindliness of disposition which procured for him the sincere esteem of all with whom he came in contact, and especially endeared him to those who enjoyed the privilege of his intimate friendship. In this Society where he was so long and so well known the many excellent qualities of head and heart which appeared so conspicuous in our late President will long be cherished with affectionate remembrance.

The Right Hon. Sir Frederick Pollock, Bart., of Scottish extraction, was born in London, September 23rd, 1783. He was the third son of Mr. David Pollock—three of whose five sons acquired great distinction in their respective callings. The eldest (Sir David) was Chief Justice of Bombay; the youngest (Sir George) is now a Field Marshal—this honour having been lately conferred upon him for his exploits in Affghanistan and

other well-fought fields. Sir Frederick, as is well known, occupied for upwards of twenty years the high position of Chief Baron of the Court of Exchequer, having succeeded Lord Abinger in the year 1844, when he was sworn a member of Her Majesty's Privy Council. Shortly after his retirement, in June, 1866, he was created a Baronet. The steps by which Sir Frederick fought his way up from a comparatively humble position to one of high distinction are interesting, as affording evidence of what may be accomplished when natural talent has coupled with it a determined will and an untiring industry.

Sir Frederick went from St. Paul's School to Trinity College, Cambridge, where he early distinguished himself, and at the end of his third term passed in the first class. Pecuniary considerations then made him think of retiring from Cambridge, but, through the kindness of his tutor, the Rev. George F. Tavel, the obstacles to his remaining at Cambridge were overcome, and this gentleman had the gratification of seeing his *protégé* come out senior wrangler of his year, 1806.

In 1807 he was called to the bar, and in three years from that date he was in extensive practice. He received a silk gown in 1827, having already attained the greatest success in his profession. For many years he led the Northern circuit, and was retained in all the most important cases. In May, 1831, he became member for Huntingdon, and was promoted, under Sir Robert Peel's administration, to the office of Attorney-General. He held his seat in Parliament until his elevation to the Bench in 1844.

Although his mind was necessarily to a great extent absorbed by the duties of his office, Sir Frederick never lost his interest for mathematical and general science; his reading in most branches of human knowledge was extensive and profound, and he ever evinced a lively interest in the most recent scientific investigations. He took, for example, a great interest in the various photographic discoveries, and although he left to his talented son (Mr. Henry Pollock) the actual practice of the art, he used all his influence to foster it, and was for many years the President of the Photographic Society. Within a few weeks of his death he was engaged on a mathematical paper, which he proposed to communicate to the Royal Society, of which he had been a fellow since February 1816, and to whose Transactions he had contributed several mathematical papers, among others, "On a Method of Proving the three leading Properties of the Ellip and Hyperbola."

A man so much otherwise engaged could not be expect to contribute directly to astronomical discovery; but he k himself always well informed of the progress of our science even to a very late period made observations with his tele When spectroscopic discoveries promised to throw lig solar physics, he acquired a spectroscope, so as to follow practically.

Sir Frederick married twice, and has left a family of twenty children. Several of his sons have already distinguished themselves. The present Sir Frederick is one of the masters of the Court of Exchequer.

The late Sir Frederick Pollock died at Hatton, on the 22nd of August, 1870, in his 87th year.

Charles Frodsham was born on April 15th, 1811. He was the third son of the late Mr. W. J. Frodsham, the eminent chronometer-maker, who, while he devoted himself to the higher branches of his own art, took an active part in the promotion of general science, and attained the honour of admission as a fellow of the Royal Society. Mr. Charles Frodsham was brought up to his father's business, and showed in his early manhood a remarkable faculty for undertaking the more minute and intricate calculations involved in the construction and regulation of chronometers. In 1847 Mr. Frodsham was presented with the Telford medal, for a paper on the Isochronism of the Balance Spring, and was also complimented on the same occasion by being made an Associate of the Institution of Civil Engineers. In 1862 Mr. Frodsham was appointed a Juror in Class XV. in the London International Exhibition, and being elected Reporter to the same, he published on the adjudication of the prizes a very clever and exhaustive report on "Chronometers, Watches, and Clocks." He was also in this same year the author of another treatise entitled "A few Facts connected with the Elements of Clock and Watchmaking." Mr. Frodsham served twice as master of the Clockmakers' Company. He was also appointed a Juror of the Dublin Exhibition in 1865, and both as Juror and Vice-President of the Paris Exhibition in 1867; although excluded by his position as Juror from all competition on these occasions, he gained eleven medals, among which was the grand gold medal of the Emperor of Russia. These various labours were achieved, and these distinctions won in spite of ill health and of an extreme delicacy of the lungs, from which he suffered through life. Mr. Frodsham was several times a member of the Council of the Royal Astronomical Society. In private life Mr. Frodsham was of a genial and lively temperament, cheerful under bodily ailments, and well stored with general information. In public life his name will ever be honourably associated throughout the civilised world with his particular art. He is justly entitled to take his place in the age in which he lived, as one of the most distinguished of English watch, clock, and chronometer makers.

Charles Mason was born on the 5th of February, 1823, at Stoke Pogis, Buckinghamshire. His only education was received at the village school. In 1846 he obtained employment in a subordinate capacity at one of the small stations of the London and Brighton Railway, and during the remainder of his life he held some office more or less important in connexion with

the railway interest. Gradually working his way, as the result of his active business habits, he, in 1855, became Superintendent of the York district of the North Eastern Railway. This appointment he subsequently exchanged for that of General Manager of the Birkenhead Railway, which he held about two years, when his successful management attracted the attention of the Directors of the London and North-Western Railway, who secured his services in the important position of Goods Manager of that undertaking. This office he retained till his appointment as Assistant General Manager of the same Company. Although Mr. Mason was a self-made man, having risen from comparative obscurity to the responsible position of sub-manager of the most influential railway company in the country, yet he was a person of cultivated mind, having always a keen taste for the beautiful in art, and an ardent love for science, especially for astronomy. His astronomical inclinations led him to seek the Fellowship of the Royal Astronomical Society, to which he was elected on the 14th of February, 1862. Mr. Mason was a first-rate man of business, and was greatly respected by all with whom he was brought into contact on railway affairs; for his frank and genial manners made him one of the most popular of railway men,and his death was universally deplored. Mr. Mason died at his residence, 22 Albert Road, Regent's Park, on the 14th of September, 1866, in the forty-seventh year of his age, leaving a wife, two sons, and a daughter, to mourn his loss. His funeral in the churchyard of Stoke Pogis was attended by more than a hundred of his railway friends, who have erected a memorial-stone over his grave as a mark of the respect in which they held him.

The late JOHN GEORGE PERRY was born on the 3rd of May, 1802. He was a pupil of the celebrated Surgeon Henry Earle, and commenced his professional career at St. Bartholomew's Hospital, of which he subsequently became a governor. For many years Mr. Perry was surgeon of the St. Marylebone Infirmary, and in 1829 he succeeded Mr. Earle as surgeon of the Foundling Hospital, a post which he held for fourteen years. In 1844 he was elected a governor, and continued a member of the committees of management of that institution and of St. Bartholomew's until his death, always taking an active part in promoting their interests.

He was for six years secretary to the Royal Medical Chirurgical Society during the presidency of Sir Benjamin Brodie. In 1843 he was appointed, at the recommendation of Sir Benjamin Brodie, an inspector of prisons, when he retired from his profession as a surgeon, much to the regret of his colleagues and friends.

In his new career he displayed the same assiduity and zeal which had always characterised him, and he received the commendations of Sir James Graham in the House of Commons for his public services. One of his most beneficial recommendations was the incorporation of a large number of the smaller borough

prisons into the county prisons, thereby placing the prisons under more efficient management, and effecting a great saving of expense. He was appointed one of the visitors of the prison for juvenile offenders at Parkhurst, and one of the commissioners of Millbank prison.

Notwithstanding his official duties, Mr. Perry took an active interest in several branches of science; for some time he was on the Council of the Zoological Society. Although his avocations prevented him from making regular astronomical observations, he, nevertheless, made himself well acquainted with astronomical phenomena, and availed himself of such opportunities as a London residence permitted for familiarising himself with such objects as could be studied with a $3\frac{1}{4}$-inch telescope. In 1860 he took part in the Himalaya Eclipse Expedition, and made some observations from his station near Burgos.

Owing to the impaired state of his health, Mr. Perry felt it necessary to resign his appointment in the summer of 1869, and after much bodily suffering he died at the age of sixty-seven, in January 1870.

Arthur Kett Barclay* was born on the 20th of June, 1806. He was educated at Harrow, having previously been instructed by a tutor, under whom he acquired much information in chemistry and geology. During the years 1829 to 1833 he made the tour of north and south Europe. In early life he was much associated with Faraday, and subsequently became a Fellow of the Astronomical, Geological, and Geographical Societies; and in 1852 was elected a Fellow of the Royal Society. At the age of twenty he was actively engaged in the brewery of Barclay, Perkins, and Co., where he continued an acting partner until nearly the end of his life.

He was one of the treasurers to the Commissioners of the Great Exhibition of 1851. He was a magistrate and deputy-lieutenant of the county of Surrey, and was also captain of the Dorking troop of yeomanry until the regiment was disbanded in 1837.

He took an active part in the politics of the country as a strong Conservative, though he would never consent to be brought forward as a candidate.

He studied astronomy for some years at Norbury, near Croydon, where he had a small observatory of his own construction. In 1848 he built the observatory at Bury Hill. The revolving dome was by Ransomes and May of Ipswich, and the equatorial 8-inch object-glass by Troughton and Simms.

Though much engaged in business and county work, he assiduously continued his observations until he was struck down by paralysis in 1855. Happily his mind was not affected, though he never again was able to continue his work; and after nearly

* This obituary notice of Mr. A. K. Barclay was received too late for insertion in the last Annual Report.

fifteen years of a precarious life, he at last sank under another attack.

Proceedings of various Observatories.

Royal Observatory, Greenwich.

The work at the Royal Observatory since the date of the last Report has been of the same character as in previous years, the Moon being considered still the most important subject of observation.

During the past year the usual meridional observations of the Sun, Moon, and Planets, have been made with the Transit Circle, whilst the Moon has been observed with the altazimuth on every available opportunity, together with a high star and the collimator for the determination of the instrumental errors.

For the last four months the observations of the small planets have been continued all through the lunation, instead of merely in the first half, as it appeared probable that no observations would be made in Paris during the continuance of the siege. This has necessarily pressed rather heavily on the observers, though no serious inconvenience has been experienced in consequence, notwithstanding the absence of Mr. Carpenter on Dr. Huggins' Oran Expedition.

Two satisfactory series of observations of the Solar Eclipse have been made with the great equatoreal and with the altazimuth respectively, the reduction of which has thrown a large amount of extra labour on the computing staff of the Observatory, though the usual reductions have proceeded without interruption. Although dense clouds obscured the Sun in the early part of the the eclipse, numerous measures of the differences of R. A. and N.P.D. of the limbs of the Sun and Moon and of the differences of N. P. D. of the cusps were taken with the former instrument during the progress of the eclipse, in accordance with a carefully-arranged plan of observation. From these the corrections to the tabular differences of R. A. and N. P. D., and to the semi-diameters of the Sun and Moon (the latter, of course, affected with twice the amount of irradiation) have been deduced with great accuracy. The errors of Hansen's Tables appear from these observations to be sensibly the same near conjunction as in other parts of the Moon's orbit.

With the altazimuth three sets of double azimuths of the Moon's second limb and six of the first limb, together with three pairs of zenith distances of the upper limb were taken while the Moon was on the Sun's face, giving the diameter in azimuth as well as the tabular error in Right Ascension and North Polar Distance. This determination of the diameter, combined with measures of the azimuthal diameter of the bright Moon previously taken, will, it is expected, give data for obtaining the amount of solar or lunar irradiation in this instrument.

The proposed observations with the Transit Circle were lost through cloud at the time of transit, which is the more to be regretted as but few observations of the Sun are available to determine the correction to his tabular place about the time of the eclipse, which is required in the differential observations with the great equatoreal in order to deduce the error of the lunar tables.

From the same cause the first còntact was lost; the times of last contact observed with various telescopes show the usual discordances due to difference of instrumental power.

The new water-telescope, for the determination of the effect on the co-efficient of aberration (as given by observations of γ *Draconis*) of the passage of the rays of light through a column of water, has been brought into working order, after some trouble experienced in securing the freedom from constraint of the object-glass, which is in contact with the upper surface of the water. A succession of cloudy days for some time prevented any regular observations with the instrument, whilst the risk of frost on the approach of winter soon rendered the removal of the water necessary until the weather should become more settled. Practically, however, the observations will be confined to those seasons of the year when γ *Draconis* passes the meridian about 6 A.M. and 6 P.M., at which time the greatest effects due to the aberration are observed. The observations will, therefore, shortly be resumed.

Ever since the erection of the transit-circle a gradual subsidence of the eastern support has been noticed, necessitating the application of a rather large correction for level. In order to remedy this inconvenience, as no adjustment of the Y is provided, about a ton weight of stone was placed on the western pier, but without producing the slightest appreciable effect—a good proof of the great stability of this instrument. As the western support could not be lowered, the plan was adopted of raising the eastern, for which purpose the Y was raised from its bed, and a sheet of thin paper, $\frac{1}{300}$-inch in thickness, placed under it, after which the level error was found to have been reduced by the quantity required, viz. 10″, whilst by means of the collimators the Y was replaced without sensibly affecting the azimuthal error.

The New Seven Year Catalogue of Stars observed with the transit circle from 1861, Jan. 1, to 1867, Dec. 31—all reduced to the epoch 1864, Jan. 1—has been printed and distributed, as well as the volume of *Greenwich Observations* for the year 1868.

Independently of its intrinsic value, as affording standard points of reference for differential observations, it is hoped that this Catalogue will prove of great value to astronomers, as giving reliable data for determining the proper motions of *all* the stars observed by Bradley, and given in Bessel's *Fundamenta Astronomiæ*, many of which have not been included in previous catalogues.

The printing of the Astronomical portion of the Volume for 1869 has been proceeding more rapidly than usual, and is now,

with the exception of some few sections, quite finished, whilst the reductions of the observations are nearly complete to the end of the year 1870.

In August last the Observatory lost the able services of Mr. Stone, on his appointment by the Admiralty to the important office of Astronomer at the Cape of Good Hope, on the resignation of Sir Thomas Maclear. He has been succeeded as chief assistant by Mr. W. H. M. Christie, B.A., of Trinity College, Cambridge.

Cambridge Observatory.

The transit instrument was dismounted in the course of the year 1869, and preparations were made for the mounting of the transit circle in the place of it.

Unexpected difficulties which Mr. Simms met with, however, in constructing the new instrument, long delayed its completion, so that it was not ready till October 1870. Mr. Simms completed the mounting of it in December; and the first assistant, Mr. Graham, is now engaged in the determination of wire intervals and other elements of reduction, and in the examination of the circle and pivots.

Prof. Adams believes that this instrument, for which the Observatory is indebted to the munificence of Miss Sheepshanks, will prove to be one of the finest of its class. The object-glass of the telescope is an excellent one, by Cooke, of 8 inches aperture. There are two divided circles of 3 feet in diameter, one of them being fixed relatively to the axis of the telescope, and the other moveable and capable of being clamped to the axis in any position. Each of the divided circles is read off by means of four micrometer microscopes, and additional microscopes, if required, may be readily applied. There are two collimating telescopes, each of 6 inches aperture, which can be directed upon each other through an opening in the central cube. A powerful apparatus is likewise provided by which the instrument may be readily and safely reversed. There are no screw adjustments for azimuth and level, but the axis of the instrument is brought into its definitive position by scraping the Y supports.

The meridian observations having been necessarily interrupted, the work done in the Observatory during the past year has been chiefly confined to the reduction of the observations and to the preparation for the press.

The Transit and Circle Observations for several years are now arranged for the press, as well as all the Equatoreal Observations of Comets and Occultations.

The printing of the Observations has commenced, and having such a large mass of materials ready, Prof. Adams hopes that it will now proceed rapidly.

The meteorological observations have been made at the usual hours of 9 A.M. and 3 P.M.

Radcliffe Observatory, Oxford.

The Radcliffe Observatory has, in the past year, as in the one preceding, suffered by the death of a valuable assistant. Mr. Béchaux, whose appointment to the office of second assistant was notified in the last Annual Report of the Society, died suddenly on the 13th of August last, of heart disease. He had devoted himself so vigorously to astronomy, that his death may be considered as affecting its interests generally as well as the Radcliffe Observatory, which has felt his loss severely. It is, however, gratifying to be informed that the vacant situation has been ably filled by Mr. Gabriel Keating, formerly employed at Greenwich and well recommended by the Astronomer Royal, and that his services are likely to be valuable to the Observatory.

The work performed during the past year is of the same character and of about the same amount as in preceding years. The 27th volume of the *Radcliffe Observations* for 1867 was published and distributed in July last, and the printing of the Astronomical portion of the volume for 1868 is nearly completed. The Catalogue of Stars, in number 1772, included in this volume, has been printed, and several copies stitched up separately, for the convenience of astronomers, to whom it may be useful. The volume will contain also about the same amount of planetary and extra-meridional observations as the preceding.

With regard to the reductions, they are in nearly the same state of forwardness as usual. The transits are completed to the end of 1870, and the North Polar Distances nearly so to the end of 1869, the entries and first steps of reduction of the latter being kept up pretty nearly to the present time. The Catalogue of Stars for 1869 is complete as far as concerns the Right Ascensions; and it only requires the North Polar Distances, when ready, to be written into it, to make it absolutely complete. It will contain about 1400 stars.

The times of beginning and ending of the recent Solar Eclipse were observed,—the latter very satisfactorily; and it may be worth while to mention with regard to the Heliometer, that Mr. Simms has recently made for it a solar eye-piece with polarising prisms for diminishing the light at pleasure. Up to the present time this splendid instrument has not been furnished with any apparatus for observing the Sun conveniently.

Stonyhurst College Observatory.

Several improvements have been carried out in this Observatory during the past year. The arrival of the large Spectroscope constructed by Simms, on the model of the one previously made by him for Mr. Huggins, led to the complete fitting-up of the Spectroscopic Studio adjoining the Equatoreal room. The battery required for the metal spectra is kept in the ceiled cellars

beneath the Observatory, so that the instruments may not be injured by the acid fumes, but cells of bichromate of potash are always ready in the studio for any occasional experiment. The room is also provided with spectrum maps, induction coil, spark apparatus, vacuum tubes, &c.

The old pulleys and bevel gearing have been removed from the Equatoreal shutter to make way for a rack and pinion.

The Equatoreal has been employed chiefly in the measurement of double stars and of the solar spectrum lines, and in the study of the prominences. Positions of new comets have always been obtained when the weather was favourable, and a careful watch kept for the periodic meteors.

An $8\frac{3}{4}$-inch Cassegrain mounted on a Tully altazimuth stand, and a 4-inch achromatic by Jones, were taken to Spain for the observation of the Solar Eclipse. The large Spectroscope was adapted to the Cassegrain, and the Jones Equatoreal was provided, in case of need, with a small Spectroscope, and a polarising eye-piece consisting of a Nicol and a quartz wedge.

In the Meteorological department the daily photographic records of the thermograph, barograph, and magnetographs, have been continued, as have also the curves for the velocity and direction of the wind. A new self-registering rain-guage made by Hicks from Beckley's design has just been received from the Meteorological Committee. There has been no break of continuity in the series of daily meteorological observations and of monthly determinations of the magnetic elements. The results are as usual printed for private circulation.

A paper has been presented to the Royal Society on the reduction of a seven-years' series of magnetic observations at Stonyhurst, and the second part of the French Magnetic Survey is almost ready for publication.

Royal Observatory, Edinburgh.

During the past year the daily time services by electric ball, gun-fire, and controlled clocks, have been continued as usual. Also the more laborious, and equally obligatory, work of computing the bi-diurnal observations collected from each of fifty-five town and country stations of the Scottish Meteorological Society, and sending the chief results in certified forms to be printed by the Registrar General of Births, Deaths, &c., in Scotland at the end of every month and quarter; and they have been printed accordingly.

The long kept up weekly observations of the carefully planned and very accurate series of the Edinburgh Observatory rock-thermometers, have recently been reduced to one uniform plan from their commencement in 1837, down to the end of 1869; and in that shape have been communicated to the Royal Society of London. A discussion of certain supra-annual cycles of tem-

perature in the Earth's surface crust, thus arrived at, accompanied the MS. figures; together with a forecast founded upon them, of the temperature characteristics to be, of the present season;— the date of communication of that forecast having been the beginning of last March.

Excepting only a few instances, the usual meridian observations of stars for place, with the transit instrument and mural circle, have been discontinued for a time; partly, because there has been only one assistant in the Observatory during a large portion of the year; and partly, in order to allow of the computation of past observations in right ascension and north polar distance being more speedily overtaken.

A Report of 34 pages with several plates (intended to appear also in the 13th volume of the *Edinburgh Astronomical Observations* now commencing at the press), addressed to the Government Board of Visitors, has been printed by the Astronomer during the past summer, and generally circulated. It contains, amongst other Observatory topics, an inquiry into the fluctuations of the position of the transit instrument on the Calton Hill, during the last twenty-nine years, and traces them up, both astronomically and experimentally, at last, it is believed, to their true source, viz., the untoward *physical* nature, touching thermotic effects, of the particular kind of stone of which the said piers are constructed; and an inquiry is therefore now much desired to be commenced into the degrees of suitability of different materials for the most exacting of the astronomical requirements of a first-class Meridian Observatory.

Glasgow Observatory.

The operations at the Glasgow Observatory continue as heretofore to be devoted chiefly to a course of star-observations, the objects selected for this purpose generally ranging between the sixth and ninth magnitudes. Some observations of the minor planets have also been made with the transit-circle during the past year, and the results are now being prepared for publication in the *Astronomische Nachrichten.*

Greenwich Mean Time has been transmitted electrically as in past years to the City and Port of Glasgow. Increased facilities have recently been given for the rating of chronometers, and the admirable advantages of Jones's invention are now appreciated by all classes of the community.

Since the beginning of the year 1868 a system of meteorological observations executed with self-registering instruments has been in operation at the Glasgow Observatory. The observations are conducted at the public expense under the superintendence of the Meteorological Committee of the Royal Society. The results of the observations are transmitted weekly to Kew, whence they are forwarded to the Central Office of the Committee in London. During the past year a Quarterly Weather Report has been pub-

lished under the superintendence of the Committee, including, among other matters of interest, a graphical delineation of the results obtained with the various self-recording instruments at the seven affiliated observatories.

Kew Observatory.

The Photoheliograph has been worked under the direction of Mr. De La Rue during the past year, the ninth of the continuous photographic observations of the Sun. From Jan. 1 to Dec. 31, 1870, 380 pictures were obtained on 220 days. During the eclipse of Dec. 22 only five pictures could be made in consequence of the unfavourable state of the weather; the heliographic positions of the Sun-spots for that day have been determined from some of the Sun pictures, and will be communicated to the Society. By means of a grant of money made by the Royal Society for that object, six sets of positive prints have been struck off for distribution from the whole series of negatives obtained during the nine years the heliograph has been at work, and completed up to date. In 1869 Messrs. De La Rue, Stewart, and Loewy presented a paper to the Royal Society (*Phil. Trans.* vol. clix. p. 1), containing a full description of the method adopted by them for ascertaining the position and areas of Sun-spots, and tables giving the areas and positions for the various Sun-spots observed during the years 1862–3 at Kew. A paper read before the Royal Society on March 10, 1870, and just published, contains the same elements for 1864–5–6; it also contains a discussion of Hofrath Schwabe's (of Dessau) collection of drawings of the solar disk from the time of their commencement in 1825 to that of their termination in 1867. The Kew heliograph will be worked until December 1871 by means of a sum voted from the Government grant of the Royal Society, when, unless some steps are taken to carry on the observations, systematic Sun-work will be discontinued in England.

Mr. Huggins' Observatory.

During the past year the Observatory has been rebuilt, and a drum of 18 feet diameter erected in place of the former dome of 12 feet diameter, to fit it to receive the large equatorial instrument constructed by Messrs. Grubb and Son for the Royal Society, and by that Society placed in the hands of Mr. Huggins.

This instrument is provided with a tube and object-glass 15 inches in diameter, and a tube fitted with a speculum of 18 inches diameter, either of which can be attached at pleasure to the declination axis of the equatorial mounting. The focal length of the object-glass, the lenses of which are united with castor-oil, is 15 feet. This instrument performs admirably in all respects and does great credit to the makers.

Spectroscopes specially adapted for the instrument and suitable

for the observation of the stars, nebulæ, and Sun, have been constructed by Messrs. Grubb and Son on the very admirable plan of automatic motion for minimum deviation recently described by them in the *Monthly Notices*.

There has not been time, as yet, to do more than make the preliminary observations for the adjustments of the instrument.

Notes on some Points connected with the Progress of Astronomy during the Past Year.

General Remarks.

The progress of Astronomy, in common with that of every other science, has been affected by the terrible war, which has so largely absorbed the attention of all classes since July, 1870. We have been influenced by it in England, but in France and Germany scientific progress has been greatly interrupted. Even before the completion, in September last, of the great cordon of troops and artillery which shut off Paris from the rest of the world, the effects of war on science were shown by the reduced size of the *Comptes Rendus*, and the disappearance of some French scientific journals. In Germany, many young astronomers of great promise, not altogether unknown in this country, exchanged the observatory for the battle-field, where some, alas! have fallen.

But notwithstanding these unfavourable circumstances, the year just closed has not been barren of discoveries. Three planets, hitherto unrecognised, have been added to the known members of the solar system; four comets have been detected, some even in Germany by an observer at no great distance from the scene of war; and an increase of our knowledge of the constitution of the Sun has been undoubtedly obtained from the recent Solar Eclipse, to observe which, two of the greatest astronomical expeditions of modern times were organised and despatched from this country and America to Sicily, Cadiz, Gibraltar, and Oran. In our own Society the evening meetings have been well attended, at which important papers—of which a list will be found on another page—have been read and freely discussed.

The Total Solar Eclipse, Dec. 22, 1870.

As this eclipse would be total at several places within easy reach of England, namely, the south of Spain, Sicily, and the north coast of Africa, it appeared to the Council an occasion on which they should take steps to assist observers, and, if necessary, organise an expedition provided with suitable instruments for attacking the important problem which still remained unsolved,

—the extent and nature of the Coronal Light. At the meeting of Council held in March, the Council resolved itself into a committee to consider the preparations to be made for the observation of the Solar Eclipse of Dec. 22. In the following month this committee united itself with a committee appointed for a similar purpose by the Royal Society. At a meeting held by this joint committee on June 16 it was resolved that the Government be solicited to grant two ships for conveyance of observers to Spain and Sicily, and also a sum of money for the preparation and transport of instruments. To this application, which was made in accordance with former usage, to the Admiralty, an unfavourable answer was received on August 10. Absence from town of some members of the joint committee and other circumstances prevented any further steps being taken until November 4, when the joint committee met and resolved that an application for means of transit for the expedition and for a pecuniary grant in aid of the funds voted by the Royal and Royal Astronomical Societies should be made to the Lords Commissioners of Her Majesty's Treasury. To this renewed application a favourable reply was returned by the Government, who placed H.M. Troop-Ship "Urgent," at the service of the expedition for the conveyance of observers and instruments to Spain and Africa, and the sum of 2000*l.* in aid of the travelling expenses of the overland party to Sicily and for the preparation and transport of instruments.

At this late moment, a few weeks only before the expedition should leave England, the greatest energy was needed to organise a party of observers and procure the special instruments needed for the proposed observations. A small organising committee was appointed, which met almost daily up to the departure of the expedition. The successful and very complete arrangements ultimately made were due in great measure to the unflagging zeal of the secretary Mr. Lockyer and of the assistant-secretary Mr. Ranyard; and the Council wish here to state how much in their opinion is owing to the valuable suggestions and assistance afforded by Prof. Stokes. The opticians, Mr. Browning, Mr. Grubb, Mr. Ladd, and Mr. Slater, afforded very valuable assistance to the expedition by the preparation and loan of instruments, for which they deserve the grateful thanks of the Society.

Distinct observing parties in charge of Mr. Lockyer, Rev. S. J. Perry, Capt. Parsons, and Mr. Huggins, were appointed for the four stations, Sicily, Cadiz, Gibraltar, and Oran. Professor Tyndall accompanied the Oran party as an independent observer.

Lord Lindsay, taking with him several skilled observers and a very complete photographic apparatus, went to Cadiz independently at his own expense.

Besides these English expeditions there was an American expedition, with Prof. Peirce at its head, consisting of two parties, one in Sicily, under Prof. Peirce himself, and one in Spain, under Prof. Winlock. Independent observations were

taken by Prof. Newcomb at Gibraltar, and by a party consisting of Profs. Hall, Eastman, and Harkness, in Sicily.

At no former eclipse have preparations been made on so complete a scale, or the work to be done so skilfully divided among observers trained to carry out efficiently the parts assigned to them. All the parties were prepared to attack the corona by the several methods of the spectroscope, the polariscope, photography, and eye-drawings. With favourable weather it was not too much to expect from these expeditions a searching and almost exhaustive examination of the coronal light by these different methods of attack.

The weather was not propitious; at all the stations the sky was more or less obscured by clouds. On the African continent, where there had been grounds for confidently anticipating a cloudless sky, the English party and M Janssen, who had escaped with his instruments from Paris in a balloon, at Oran, and Drs. Weiss and Oppolzer at Tunis, saw nothing of the eclipse at the time of totality, though the earlier phases were visible at Oran.

At Cadiz and in Sicily successful photographs of the totality were obtained by Lord Lindsay, Mr. Willard, of the American Expedition, and Mr. Brothers. At these stations, and also at Estepona, some observations were obtained of the spectrum and polarization of the corona.

Although the gain to our knowledge of Solar physics is much less full and decided than doubtless it would have been if the very efficiently equipped and competent observers had been favoured with a cloudless sky, the new information which comes to us from the eclipse is very valuable, and well repays the large amount of thought, time, and money, which were so freely bestowed upon the preparations.

The present time is too early for a complete analysis of the different observations with a view of eliciting from them the new teaching which they may contain of the extent and nature of the coronal light, still it may not be undesirable to give a short account of some of the more important observations.

In the last Annual Report, in the account of the Eclipse of August, 1869, attention was called to the two apparently distinct portions besides the prominences in the light seen round the Moon during totality. The American pictures showed similar indications of brighter portions near the Sun's limb, within which the eruptions of hydrogen forming the prominences take place, to those which were visible in the photographs taken by Mr. De La Rue in 1860, and by Major Tennant and Dr. Vogel in 1868. A distinction between different portions of the coronal light was observed as early as 1706 by MM. Plantade and Capiés at Montpelier. "As soon as the Sun was eclipsed there appeared around the Moon a very white light forming a corona, the breadth of which was equal to about 3′. Within these limits the light was everywhere equally vivid, but beyond the exterior contour it was less intense and was seen to fade off gradually into the surrounding darkness, forming

an annulus around the Moon of about 8′ in diameter." In 1842 M. Arago considered this distinction to be sufficiently marked to sanction the subdivision of the corona into two concentric zones, the inner zone equally bright and well defined at the outer border, while the exterior zone gradually diminished in brightness until it was lost in the surrounding darkness.

The observations of the eclipse of last December confirm these earlier descriptions as to the apparent subdivision of the coronal light, though the breadth of the inner zone varies considerably as described by different observers. In our future remarks we shall restrict the word *corona* to the inner brighter ring, and for the faint exterior portion use the term *halo*.

It may conduce to clearness in our interpretation of those observations which appear to differ from each other, if we consider that the imperfect transparency of our atmosphere must cause a scattering of a portion of the light of the corona seen through it, and form a more or less brightly illuminated screen between the eye and the eclipsed Sun. This atmospheric light will interfere especially with the observer's appreciation of the form and extent of the faint halo. There may exist at least three distinct sources of the light seen about the Sun, in addition to the prominences, the corona, a solar halo overlapping the corona or beginning at its exterior limit, and an atmospheric halo produced by the scattering of the light by our atmosphere. The corona and solar halo would probably not alter greatly in the short time between observations of the same eclipse at different stations, but the scattering of light would be peculiar to each station, and be mixed up with the effect of haze or light cloud present at the time. It is *possible* that without the Earth's atmosphere, some scattering of light may arise from the imperfect transparency of interplanetary space, not to speak of the possible existence of finely divided matter more densely aggregated in the neighbourhood of the Sun. It may be that in these and some other considerations will be found the key to the interpretation of the widely different descriptions of the solar surroundings which come to us from different observers.

Prof. Watson observing at Carlentini describes a bright corona about 5′ high, observations at Cadiz give a breadth of about 3′; Lieut. Brown observing with Lord Lindsay found the inner zone which he saw defined at its outer margin to vary from 2′ to 5′ in breadth. Mr. Abbatt at Gibraltar at about 5′ high. Some of the observers describe the exterior contour of the corona to be affected by the prominences bulging out over the loftiest of these. In the photographs a defined zone is also seen,—in Lord Lindsay's photographs and the one taken by Mr. Willard, it extends rather more than 1′. In the photograph by Mr. Brothers the height of the brighter zone varies from 3′ to 5′.

We will now speak of the photographs of the totality, which are very instructive.

The photographs taken at Cadiz by Lord Lindsay were ob-

tained by placing the sensitive surface at the focus of a silvered glass mirror 12¼ inches in diameter and 6 feet focal length, giving an image of the Sun about ¾-inch in diameter. The other photograph taken near Cadiz by Mr. Willard of the American expedition, was obtained at the focus of an achromatic object-glass of 6 inches diameter, specially corrected for actinic rays.

Mr. Brothers, at Syracuse, employed a photographic object-glass of 30-inches focal length and 4 inches diameter, lent to him by the maker, Mr. Dallmeyer.* This lens gave a brilliant image of the Sun about three-tenths of an inch in diameter, which was received upon a plate 5 inches square. The camera was mounted on the Sheepshanks equatoreal, belonging to the Society.

The photograph taken at the commencement of totality by Lord Lindsay had an exposure of twenty seconds. It shows around the Moon's advancing limb a bright corona extending about 1′ from the Moon's limb, in which the prominences are distinctly marked, and outside this a halo of faint light diminishing rapidly in brilliancy with indications of a radial structure which can be traced as far as 15′ from the Moon's limb. On the other side of the Moon, where it overlaps the Sun sufficiently to conceal the prominences and the bright corona, *the halo is almost absent.* It may be suggested that such portion of the halo as appears around the advancing limb of the Moon has its origin on this side of the Moon. As a pure speculation the explanation may perhaps be hazarded, that the true solar halo, as some spectroscopic observations would suggest, was less powerfully actinic than the scattered light of the prominences and corona, in which the halo on the one side of the Moon only as seen on the plate may have its origin.

The photograph taken by Mr. Willard was exposed during a minute and a half, and therefore must contain mixed up several successive appearances. The prominences are distinctly shown, and a defined corona of rather more than 1′ in height. In the halo there are indications of portions of unequal brightness, and a radial structure, but the most remarkable feature is a V-shaped rift or dark space in the halo on the south-east, beginning from the outer boundary of the bright corona, a second similar dark space is faintly traceable on the south. The same dark gaps are also recorded in an eye-sketch by Lieut. Brown. Similar dark rifts are also shown in Mr. Brothers' photograph taken at Syracuse. The photograph taken by Mr. Brothers is very valuable, since it shows the halo extending towards the north-west, about two diameters of the Moon, and on the east and south about one diameter, the halo, therefore, is not concentric with either the Sun or Moon, but extends to the greatest distance in the direction from which the Moon is moving. It shows in many parts traces of a radial structure. The stronger light about the Moon is much broader on the west and north-west, and assumes a somewhat stellate

* These lenses are constructed by Mr. Dallmeyer for photographic copying.

appearance with rays gradually softening down as if combed out into the fainter halo. This photograph was taken in eight seconds, from the 93rd to the 101st second after the commencement of totality, and therefore presents a true representation of the different phenomena at the time, that is, as regards their relative actinic power which may possibly differ in a sensible degree from the relative brightness they present to the eye. The eye-sketches made at different stations show remarkable differences, especially in the form of the outer part of the halo; some represent it as consisting of separate rays, others give to it an almost true geometrical contour; in some of the Spanish sketches a tendency to assume a roughly quadrangular form can be detected, while in most of the Sicilian drawings there is a tendency to an annular form.

We pass to the spectroscopic observations of the corona and halo.

Prof. Winlock, using a spectroscope of two prisms on a five and a half inch achromatic, found a faint continuous spectrum. Of the bright lines, the most persistent was 1474 Kirchhoff. This bright line, and the continuous spectrum without dark lines, were followed from the Sun to at least 20′ from his disk. Prof. Young estimates the least extension of this line to a solar radius.

Capt. Maclear, observing with a direct vision spectroscope attached to a four-inch telescope, saw a faint continuous spectrum and bright lines in positions about C D E and F to a distance of 8′ from the Moon's limb, and also the same lines, but much fainter, *on the Moon's disk.* This observation would seem to show, as has been already suggested, that some of the light from the true surroundings of the Sun is scattered by some medium between the eye and the Moon, and therefore the distance from the Moon to which these lines can be traced does not imply necessarily an equally great extension of the true halo.

Lieut. Brown, of Lord Lindsay's party, saw only a continuous spectrum without bright lines, from $4\frac{1}{2}'$ to $25'$ from the Moon's limb. Mr. Carpmael, observing at Estepona, saw three bright lines in the spectrum of the corona. He considers the one in the green to correspond with 1359 Kirchhoff.

The observations with the polariscope show that a portion of the coronal light is polarized; and though the results as to the plane of polarization are interpreted differently by different observers, there seems reason to suppose with Mr. Ranyard and Mr. Peirce that the light is polarized radially, showing that the corona and halo may possibly reflect solar light as well as emit light of their own.

There is one observation made by Prof. Young which is of so much importance that it will be well to give an account of it in Prof. Langley's words:—

"With the slit of his spectroscope placed longitudinally at the moment of obscuration, and for one or two seconds later, the field of the instrument was filled with bright lines. As far as could *be judged,* during this brief interval every non-atmospheric line

of the solar spectrum showed bright; an interesting observation confirmed by Mr. Pye, a young gentleman whose voluntary aid proved of much service. From the concurrence of these independent observations we seem to be justified in assuming the probable existence of an envelope surrounding the photosphere, and beneath the chromosphere, usually so called, whose thickness must be limited to two or three seconds of arc, and which gives a discontinuous spectrum consisting of all, or nearly all, the Fraunhofer lines showing them, that is, *bright* on a dark ground."

Rapid and imperfect as this early sketch must necessarily be of the observations of the last eclipse, it shows a distinct and important gain to our knowledge of solar physics.

Spectrum Analysis.

In the winter of 1867–8 Angström found the light of the auroral arch to be nearly monochromatic, giving in its spectrum a single brilliant line in the green near the group of calcium lines, and traces of three feeble bands near F. This observation was confirmed by Struve in 1868. In 1869, Prof. Winlock observed five bright lines in the green and blue parts of the auroral spectrum.

During the past year a brilliant line in the red portion of the spectrum has been detected in some parts of the auroral display. This line was observed first by Mr. Ellery at Melbourne, on April 5, 1870. "The red streamers," he writes, "were gorgeous, and emitted light enough to read a newspaper by. The most remarkable and brightest of the lines in the spectrum was a red line more refrangible than C; a greenish band or two in the position of the green calcium lines, and a cloudy band more refrangible, appeared as if irresolvable into lines. The dark segment rested on the sea-horizon. Above this was an arch of greenish auroral light, and from a well-defined boundary of this the rose-coloured streamers started zenithwards. The red line disappeared immediately the spectroscope was directed to any point below this boundary, and only the green lines remained. The loss and reappearance of the red line was as sharp as possible as the slit passed from the red to the green region."

On October 25 Zöllner compared the position of the red line with the lines of lithium and sodium From the position of the auroral line relatively to these, he considers that it falls in the spectrum very nearly where a group of atmospheric lines occurs in the solar spectrum having a mean wave-length of 0·0006279. Zöllner suggests that this auroral spectrum, which does not correspond with any known spectrum of the gases of the atmosphere, may be a spectrum of one or more of these gases of an order we have not yet been able to obtain experimentally, since we can have to do only with thin strata of gas, whereas the auroral light may come from an enormously thick layer of one or more of the gases of the atmosphere at a relatively low temperature.

Our Fellow, Mr. Lockyer, in continuation of his important researches on Solar Physics, considers that he has now evidence, from the different behaviour of the line C and the line near D, that the latter does not belong to hydrogen—a result in harmony with the absence of the line near D, from the different spectra of hydrogen obtained experimentally.

His observations show, he believes, that prominences may be divided into two classes,—those in which great action is going on and lower vapours injected, and those which are tranquil so far as wave-length goes, which are usually high, bright, and persistent.

While observing a solar spot with the spectroscope, on April 16, Mr. Lockyer saw "the whole prominence spectrum was built up of single discharges shot out from the region near the limb, with a velocity sometimes amounting to a hundred miles in a second. On the following day, using a tangential slit, he found "in the spectrum of the base of the prominence hundreds of the Fraunhofer lines beautifully bright." Mr. Lockyer considers that he has evidence that at the present maximum period of sun-spots not only is the region of a spot comprised by the penumbra, but the chromosphere also is shallower than in the year 1868.

Prof. C. A. Young has succeeded, by means of a spectroscope having a dispersive power of thirteen prisms of heavy flint each with an angle of 55°, attached to an achromatic telescope of 6·4-inches aperture, and 9 feet focal length, in obtaining photographs of the solar prominences. Negatives have been made which show clearly the presence and general form of the protuberances, but at present the definition of details is unsatisfactory. The hydrogen line γ (2796 of Kirchhoff), though very faint to the eye, was found to be decidedly superior to F in actinic power.

On Sept. 14, Prof. Young saw a very brilliant small fragment detach itself from a prominence, enlarging in size, and growing fainter as it rose. It disappeared in 12½ minutes at a distance of 2′ 30″ above the limb of the Sun. Allowing for the obliquity of motion to the parallel of declination, the length of path passed over by this cloud was more than 90,000 miles, with a velocity of above 120 miles per second.

Prof. Young observed on Sept. 27 a prominence formed of separate, well-defined narrow streamers, which appeared to consist of matter first violently ejected and then as violently deflected by some force acting nearly at right angles. He says:—"I am unable to see how any mere projection from the Sun could have produced such a form, and cannot help feeling that it indicates a something in which powerful currents may exist, even at such great elevations above the solar surface. In short, an atmosphere extending far beyond the limits which calculation would seem to assign as possible."

Prof. Young calls special attention "to a bright line 2581·5, Kirchhoff, the only one of my list which is not given on Mr. Lockyer's. This line, which was conspicuous at the eclipse, 1869, seems to be always present in the spectrum of the chromosphere,

and shows the form of its upper surface or of a protuberance nearly as well, though, of course, not so brightly as the line 2796. It has no corresponding dark line in the ordinary solar spectrum, and not improbably may be due to the same substance that produces D."

Professor Respighi has done good service in the same field of research by mapping all the prominences which appear around the Sun from day to day, and arranging them for each day in a straight line, so that the appearances for different days by being placed under each other admit of easy comparison. From the conclusions to which this method of studying the forms of the prominences has led him, we may select one or two. "The prominences are less active, less frequent, and less developed at the equator than towards the poles, while in the circumpolar regions great protuberances are not met with, but only very small, temporary flames,—the prominences seem to be, therefore, in connexion with the spot-zones, and possibly to be influenced by the solar rotation." "The difference of duration of the prominences is very great, while some develope and disappear in a few minutes, others remain visible for many days."

Mr. J. H. Hennessy, to whom a spectroscope was intrusted by the Royal Society for observations of the atmospheric lines of the Solar Spectrum at different altitudes of the Sun at the favourable position of Mussoorie, has sent in a first report of his observations, together with a chart of the atmospheric lines, as seen by him at sunset. This map has been printed in the *Proceedings of the Royal Society*, and may be found of assistance to those who are studying these lines.

Valuable additions have been made during the past year to the apparatus employed in spectroscopic research. The most important of these doubtless are improved methods by which the prisms of a spectroscope can be brought automatically to the position of minimum duration for any part of the spectrum to which the observing telescope is directed. By such an arrangement the spectrum is rendered more nearly normal with respect to the relative extension of the different colours, but the most important advantage is the greater brightness of the spectrum, precisely where light is most needed, towards the violet and red limits of the visible spectrum, in consequence of the whole of the light from the collimator being enabled to reach the observing telescope. Instruments possessing these advantages in a greater or less degree had been constructed by Littrow, Rutherford, Prof. Young, and Mr. Lockyer. We must refer to the *Monthly Notices* for a description of independent methods by which this important addition to the spectroscope has been successfully accomplished by Mr. Browning and Mr. Grubb, and for the valuable suggestions contained in a paper by Mr. Proctor.

Attention may also be called to new forms of registering spectroscopes constructed for recording rapidly the positions of lines which might be seen during observations of the late solar eclipse. Professor Winlock contrived a form of instrument in which the positions of the observing telescope when directed to different parts of the spectrum are recorded by marks upon a plate of silvered copper.

In an instrument constructed by Mr. Grubb at the suggestion of Mr. Huggins, the observing telescope is fixed, and a pointer placed at its focus can be brought rapidly to any part of the spectrum by a quick motion screw, a positive eye-piece being used, which is movable, so that the line under observation can be brought into the middle of the field. The arm carrying the pointer is connected by a lever with a second arm carrying two needles, which pass over a card contained in a frame, which may be brought successively into five new positions, for the record of as many spectra. By pressing a finger on one of the needles, a prick on the card records the position of the line. If the line is bright, the second needle is also pressed, by which a second prick is made below the one recording the position of the line, and in this way a bright line is distinguished from dark lines which may have been recorded.

The Minor Planets.

Three minor planets have been discovered within the last twelve months, *Lydia* (110) at Marseilles, by M. Borelly, on April 19; *Ate* (111) at Hamilton College, Clinton, New York, by Dr. C. H. F. Peters, on August 14; and *Iphigenia* (112) also by Dr. C. H. F. Peters, on September 19. The mean distance of each from the Sun, assuming the radius of the Earth's orbit = 1, is for *Lydia* 2·693, *Ate* 2·576, and *Iphigenia* 2·436. The three new members are therefore included among those composing the innermost portion of the asteroidal ring between *Mars* and *Jupiter*. The mean distance of *Flora*, the nearest of the minor planets, is 2·201, and that of *Sylvia*, the most distant, is 3·494.

The *Berliner Jahrbuch* for 1873 contains a supplement giving approximate ephemerides for 1871, at 20-day intervals, for 108 of the minor planets. The places are sufficiently accurate for finding them with the equatoreal. Daily ephemerides for 58 are also given when the planets are near opposition. As astronomers have now to rely, with few exceptions, on the *Berliner Jahrbuch* for the tabular places of the minor planets, it would be very advantageous to meridional observers if errors of the ephemerides were published in the *Monthly Notices* or the *Astronomische Nachrichten* as soon as possible after equatoreal observations have been made. The identification of the smallest of these minute objects is always a matter of difficulty when observed in a telescope with a field full of objects of a similar magnitude.

The minor planets continue to be systematically observed at

Greenwich, Paris, Königsberg, Leipzig, Leyden, Washington, the Observatory of Hamilton College, Clinton, New York, and a few others. Since the closing of Paris by the siege, they have been observed at Greenwich throughout the lunation when practicable, instead of only during the period from new to full Moon. The very cloudy weather of the last few months has, however, very seriously interfered with this class of observations.

Recent numbers of the *Astronomische Nachrichten* contain detailed investigations of the orbits of *Thalia*, *Polyhymnia*, and *Euphrosyne*, by Mr. Schubert, of the American Nautical Almanac Office, to whom Astronomy is already indebted for many similar researches. M. Oppolzer has also contributed a paper on the Orbit of *Olympia* (*Elpis*), in which he has availed himself of all the observations of that planet made since its discovery in 1860.

Discovery of Comets.

The following comets have been under observation since the date of the last Report. Three of them were detected by our Associate, Dr. Winnecke, whom the Council are glad to see restored to active work after his long illness.

1. Comet I. 1870, discovered by Dr. Winnecke, at Carlsruhe, in May 29.
2. Comet II. 1870, discovered by M. Coggia, at Marseilles, on August 28.
3. Comet III. 1870 (Periodic Comet of D'Arrest), detected by Dr. Winnecke, on August 31.
4. Comet IV. 1870, discovered by Dr. Winnecke, on November 23.

All these comets have been well observed, especially at the German observatories. The elements of their orbits, as well as the separate observations, have been published in the *Astronomische Nachrichten.* Some observations of Comet II. 1870, by Mr. Joynson, are inserted in the *Monthly Notices* for December.

The New Zenith Sector of the Indian Survey.

The zenith sector constructed by Messrs. Troughton and Simms from the designs of Colonel Strange, alluded to in the last Annual Report as having been forwarded to India, has been practically tested by observations of stars in the ordinary routine work of the Indian Great Trigonometrical Survey. After the scrupulous attention given by Colonel Strange to every detail required in the construction of the instrument, some of which were of a novel character, it must be very gratifying to him and also to those who inspected the sector before it left England, to find that the promise of good work given by the preliminary trials at the observatory of the Indian Store Department, has been fulfilled to the satisfaction of the observer, Captain Herschel, by whom the observations in India have been made. As an

illustration of the general accuracy of the determinations of absolute latitudes by the use of the new zenith sector, Captain Herschel remarks that one night's observation (of thirty-six stars in six hours) is amply sufficient to give a resulting latitude, of which the probable error is not greater than $0''\cdot2$. Referring to a recent determination described in a communication to Colonel Strange, he further remarks that "at the last station twenty-two stars out of the list were *Nautical Almanac* stars, so that the latitude was easily deducible. Cutting out *Procyon*, which has a large proper motion, and, I suspect, an erroneous place; and *γ' Virginis*, which has almost certainly an erroneous place, due to confusion of the two components, the remaining twenty give latitudes all falling within a range of $1''\cdot6$." Captain Herschel is of opinion that this satisfactory agreement is due entirely to the great stability of the instrument, and not to any unusual skill on the part of the observer. The Council have great pleasure in congratulating Colonel Strange on the success attending the active employment of this the first of the two proposed zenith sectors to be furnished for the use of the Indian Survey, the construction and examination of which occupied his attention for a considerable time. There seems but little doubt that in their new zenith sector, the officers of the Indian Survey possess a sensibly perfect field instrument for the determination of terrestrial latitudes, and for other geodetical purposes.

The Graphical Construction of a Solar Eclipse.

Among the many valuable astro-mathematical investigations for which the Society is indebted to Prof. Cayley, that on the Graphical Construction of a Solar Eclipse is not the least important. In addition to a complete theoretical discussion, explaining every detail connected with the subject, an example is appended to show that the method can be adapted to practical use. Prof. Cayley has chosen for his example the eclipse of the Sun of December 21–22, 1870. Taking the times and places of the beginning and end of the eclipse, it appears that the error from his construction amounts only to 1½ minute of time and about 1½ degree of place. Prof. Cayley, however, considers that a far greater accuracy than this might be obtained, if a more complete blank projection had been employed by him, and more care taken consequently in the construction. If this were done, then there is but little doubt that a diagram of an eclipse could be constructed which would bear a favourable comparison with the diagrams inserted in the eclipse section of the *Nautical Almanac.*

Mr. Proctor's New Star-Atlas.

For the use of an astronomical instrument a star-catalogue is absolutely necessary; but when an acquaintance with the

separate constellations, or with the general configuration of the principal stars is desired, recourse must be had either to a globe or to a good star-atlas. The valuable star-charts which have been published within the last thirty years, such as those of Argelander, Chacornac, Dien, the Berlin maps, and Mr. Bishop's charts, are useful only to the astronomer who is in the habit of comparing for special purposes the small stars in the field of his telescope with their recorded positions in the chart. These representations of the heavens include stars down to the ninth magnitude, while in special districts of limited extent stars no brighter than the minor planets have been entered. For reference to the relative positions of the brighter stars, however, these charts help us little; and it was to supply this want that our indefatigable Fellow, Mr. Proctor, published in a series of twelve maps a representation of the heavens arranged in such a manner as to exhibit, without sensible distortion of the constellations, all the stars contained in the British Association Catalogue, all the nebulæ in Sir John Herschel's catalogue down to the order marked very bright, all the binaries and suspected binaries in Mr. Brothers' catalogue; all the objects in Admiral Smyth's Celestial Cycle; all the red stars contained in Dr. Schjellerup's catalogue; and, finally, all the recognised variable stars. Two index-plates are also given in which the constellation-figures are inserted.

The maps were drawn originally on the scale of a 30-inch globe, and subsequently reduced by Mr. Brothers, of Manchester, by photo-lithography. Although there is a want of that finish which a professional lithographer would probably have given to the lines and writing, yet that is of small moment in comparison with the preservation of accuracy obtained by the use of photography. The maps are, therefore, exact copies, on a reduced scale, of Mr. Proctor's work as performed by his own hand. The work is dedicated to the memory of our late esteemed President, Admiral Manners.

The Orbit of α Centauri.

The brilliant binary of the southern hemisphere, *α Centauri*, has occupied the attention of observers, at intervals, from the date of Feuillée's observation at Lima, in July 4, 1709, to the present time, either in observations for the investigation of its annual parallax, or for the determination of the elements of its orbit. Recently Mr. E. B. Powell, of Madras, has published revised elements of the orbit of *α Centauri*, determined from a series of recent observations made by himself at a favourable part of the stellar orbit, when the extremity of the perspective ellipse corresponded to the lesser maximum of distance. Mr. Powell is so well satisfied with his observations and deductions as to remark:— 'Now, however, though even four or five additional years will enable us to improve the orbit, especially as to the time of periastral passage, the results I have arrived at undoubtedly approxi-

mate *pretty closely* to the truth." From one hundred observations on thirteen nights the angle of position for the epoch 1870·1 was found to be 20°·27′, and from 162 observations on twelve nights, the angular distance equalled 10″·24. The computed period of revolution is 76·25 years. It is the intention of Mr. Powell to lay the complete details of these observations before the Society in a future paper.

Distribution and Distances of the Fixed Stars.

There is no section of astronomical speculation more fascinating than that relating to the probable laws which have governed the distribution and relative distances of the stars. The late Professor Struve, in his remarkable and classical treatise *Etudes d'Astronomie Stellaire*, put forward a general theory founded upon an investigation of the stars observed by Bessel in zones, and catalogued by M. Weiss. The general conclusions of M. Struve were accepted by the principal astronomers as a theory reasonable and probable, although the application of his theory was necessarily liable to some irregularities. So great was the approval of M. Struve's speculations on the relative distances of the stars, which were classified by him into groups, according to magnitude,—stars of the first magnitude being the first group and the nearest to our solar system,—that his results were adopted in many subsequent stellar investigations, especially by the Astronomer Royal, in his first paper on the "Movement of the Solar System in Space," and also in the second, which was written at his request by Mr. Dunkin.

Since the publication of the theoretical conclusions of the two Herschels and Professor Struve, the subject has remained somewhat in abeyance till Mr. Proctor, during the construction of some of his star-maps, was impressed with certain apparent laws of stellar distribution and aggregations, presented by the stars visible to the naked eye. He believes that if a systematic study of the general distribution of the smaller stars were made, still more remarkable laws of aggregation would be probably revealed. For example, if the 310,000 stars contained in Argelander's charts were mapped upon the interior surface of a hollow globe, together with all the known nebulæ, the results of Herschel's star-gauges, and an accurate representation of the Milky Way and Magellanic Clouds, a larger amount of light would be thrown on the relations between the members of the sidereal system than could be obtained by direct observation, even with the most powerful instruments.

Among the numerous papers read before the Society during the last year by Mr. Proctor, two on this subject may be referred to. The first is entitled, "On the Resolvability of Star-Groups, regarded as a test of Distance;" and the second, "The Laws according to which the Stars visible to the naked eye are distributed over the Heavens." In the first of these papers,

Mr. Proctor expresses an opinion that the mere clustering aggregation of unresolved stars is no evidence that the cluster is at a relatively immense distance. He supposes such a cluster to consist of a number of stars all equal in magnitude, and separated from each other by equal intervals, so that each star could be recognised. If the cluster were rapidly swept away into space, the distances between the stars would become smaller as the group gradually disappeared. If the intervals became too small to be distinguished, and the stars remained visible, the result would be a nebulosity. But, on the contrary, if the distances between the stars were sufficiently great to permit each to be seen separately at the moment of disappearance, then the cluster could never become nebulous. Mr. Proctor, therefore, remarks, "that the nebulosity of a star-group whose members are equal and equally distributed, is a question not of distance merely, but of constitution, of the relation between the size and brightness of the constituent orbs and the distances which separate them from each other."

The second paper discusses the relative distribution of the visible stars over the heavens. Mr. Proctor has divided the celestial sphere into areas of different dimensions, the largest being $\frac{6}{11}$ of the area of the hemisphere, and the smaller $\frac{1}{62}$ part, the latter being the gaps or lacunæ in the Milky Way. A very rich region of lucid stars covers *Cygnus*, *Cephcus*, *Ursa Minor*, and *Lacerta*, in the northern hemisphere, while a corresponding rich region covers the keel of *Argo Navis* in the southern hemisphere. But the richest region to be found in both hemispheres is the Milky Way. From a consideration of the comparative number of lucid stars contained in each of the thirteen regions into which Mr. Proctor has divided the heavens, he shows that among the lucid stars special laws of aggregation and segregation exist; and as the lacunæ in the Milky Way are exceptionally bare, while the Milky Way itself includes the richest region of lucid stars, "the fact seems," says Mr. Proctor, "to dispose beyond all question of the theory that the stars forming the milky light of the galaxy (regarded as a whole) lie at vast distances beyond these lucid stars."

Although the Council are not at present in a position to offer any opinion on the stellar speculations which Mr. Proctor has brought before the Society, yet they acknowledge the originality of thought which has been exhibited in these and other papers by him on the probable constitution of the universe.

Dunsink Observations.

During the past year the Astronomer Royal for Ireland has published a report of his observations from 1868 to 1869 with the South refractor. The object-glass of this instrument, 11¾ inches diameter, was the gift of the late Sir James South. The

lens was mounted equatorially by Messrs. Grubb and Son in a very convenient observatory in 1868. The first paper in Dr. Brünnow's report contains a new investigation of the parallax of *α Lyræ*. This investigation rests upon a number of observations of the distance and angle of position of the small companion, the parallax obtained represents therefore the difference of the parallax of the two stars; but as the small star does not participate in the proper motion of *α Lyræ*, Dr. Brünnow considers that its parallax, if it could be taken into account, probably would not alter the parallax assigned to the large star by one hundredth of a second. The value of π comes out $+0''\cdot2143$ with a probable error of $\pm0''\cdot0095$. Dr. Brünnow intends to check this result by the method of observing differences of declination.

The second investigation consists of the determination of the parallax of 61 *σ Draconis* from comparison with a star of the tenth magnitude which is situated 1′ north, and follows it at $1^m\ 52^s$. The result comes out $\pi = +0''\cdot225$ with a probable error of $\pm 0''\cdot0279$. As this value is derived from comparison with only one star, it is possible, though by no means likely, that the periodical variation of the observed differences of declination may not be really due to parallax, but may be attributable to some unknown cause, having nearly the same period which affected the instruments. Dr. Brünnow is engaged in an additional series of observations which will remove any doubt which might arise on this point.

The report, which contains in addition to these investigations a table of micrometrical measures of double stars and a description accompanied by plates of the newly erected refractor and observatory, does great credit to the industry and zeal of the Dunsink astronomer.

Heat of the Moon.

Lord Rosse has continued successfully his research on the heat of the Moon, and has confirmed his earlier observations of which some account appeared in the last Annual Report. He now finds the percentage of the Moon's heat transmitted by a plate of glass to be 12, while the same plate gives passage to 87 per cent of the solar heat, and only 1·6 per cent of the heat from a body at a temperature of 180°. In these experiments on lunar radiation as the quantity of heat measured by the thermopile represents the difference between the radiation of a circle of sky containing the Moon's disk and a circle of equal diameter of neighbouring sky, the absolute temperature of neither the Moon nor the sky is obtained. In order to obtain information on these points, Lord Rosse has endeavoured to connect the radiation of the sky with that of a body of known temperature, his observations for the apparent temperature of the sky give values varying from 16° to 31°.

Lord Rosse finds that within a limited range of altitude the

heating power of the Moon's rays appears to diminish with the altitude only about one third as fast as the intensity of the solar chemical rays, as ascertained by Roscoe and Thorpe. The Moon's light was observed to diminish with the altitude in the proportion of three to one, but the lunar heat in the proportion of about five to one. As far as can be ascertained from these observations, the maximum of heat seems to occur a little after full Moon.

Colour of Jupiter.

During the last and present oppositions of *Jupiter* colours and markings have been observed on its surface, especially on the belts, by Mr. Browning, and by Dr. Mayer, of Philadelphia. Browning suggests that the coloured appearances may be of a periodic nature; it is hoped therefore that as *Jupiter* is now nearly at its greatest north declination, and peculiarly favourable for observation in these latitudes, that a systematic scrutiny of its surface will be undertaken by observers in possession of telescopes of superior optical power. It will be very important to have careful and trustworthy delineations of the planet's disk made from time to time, so that materials may accumulate for the comparison of the appearances of the planet at different epochs.

Royal Observatory, Cape of Good Hope.

Many of the Fellows are aware that Sir Thomas Maclear has resigned the office of Government Astronomer at the Cape of Good Hope, in consequence of declining health, and that the vacancy has been filled up by the appointment of our late valued Secretary, E. J. Stone, Esq.

In the face of the large accumulation of unreduced observations, it appeared to Mr. Stone necessary to somewhat restrict the observing with the transit circle. The observations of stars near the pole had been somewhat confined to close circumpolars. It appeared desirable therefore to extend these observations to stars further from the pole for a check upon the accuracy of the refraction tables employed and the colatitude adopted. It has been thought also that this extension of zero points near the South Pole may be valuable for many purposes, and amongst others for the adjustment of instruments in 1874. A series of such observations has been in progress since October and will be continued for some time. With the present year a systematic reobservation of Sir John Herschel's Cape observations of double stars has been commenced. This will not throw much additional computing labour upon the staff. The first aim of the staff will be to bring forward as soon as possible a standard catalogue of stars, observed with the great transit circle during the years 1856 to 1864. The reductions of the observations made in 1856 have been completed, and it is hoped that the results will shortly be

in the hands of astronomers. The catalogue will contain the places of 328 stars, all well observed. Great praise is due to the assistants generally, but more particularly to Mr. Mann, for the energy with which the somewhat uninteresting and irksome labour of bringing up the heavy accumulation of back work is now being pressed forward.

The vigour with which Mr. Stone has commenced the examination of the great mass of arrears of computations, gives the Council every reason to hope that we shall soon have a standard catalogue of southern stars observed with an instrument comparable with the Greenwich transit circle.

Argentine National Observatory.

The Fellows will receive with gratification the intelligence that an addition has been made to the comparatively small number of astronomical observatories in the Southern hemisphere, and that the new establishment has been placed under the able directorship of our Associate, Dr. B. A. Gould. The Argentine Congress having voted the foundation of a National Observatory at Cordova ($31\frac{1}{2}^\circ$ South latitude), Dr. Gould was invited by the Minister of Public Instruction to organise and take charge of it, pursuing such a scheme of observations as he considered most desirable. Dr. Gould came to Europe in the spring of the past year to procure instruments and books, and went to Cordova in the course of the summer, taking with him four assistants. His first aim will be to extend the survey of the Southern heavens from the limit reached by the zone observations of Argelander (30° S. Decl.) towards the limit of certain as yet unreduced zone observations made by the late Commander Gilliss, extending to about 30° South Polar Distance. All stars down to the ninth magnitude will be included in this survey, and their positions will for the present be made to depend upon the standard star-places determined by Dr. Gould, which form the basis of those given in the American *Nautical Almanac*. Physical observations will be undertaken as far as may be consistent with the energetic prosecution of the before-mentioned task. The Observatory is, or will be, furnished with a Repsold Meridian Circle of $4\frac{1}{2}$-inches aperture, an equatoreal of 11 inches aperture, a spectroscope, and a Zollner's astrophotometer.

Papers read before the Society from February 1870, *to February* 1871.

1870.

Mar. 11. Dark objects crossing the Sun's disk. Lieut. Herschel.
Ephemeris of the Satellites of *Uranus*. Mr. Marth.
Occultation of *m Tauri*. Capt. Noble.
Observation of *Venus* near inferior Conjunction. Capt. Noble.
The Zodiacal Light. Capt. Noble.
On further Changes in the Coloured Belt of *Jupiter*. Mr. Browning.
On the Corona and Zodiacal Light, with suggestions respecting the Observations to be made in December next. Mr. Proctor.

April 8. Observations of Lunar Eclipse, Jan. 7, 1870. Mr. Tebbutt, Jun.
On the Floor of *Plato*. Mr. Birt.
On the Orbit of the Comet of 1683. Mr. Plummer.
On the frequency of Sun-spots, and their Connection with the Magnetic declination variation. Prof. Wolf.

May 13. On some Photographs taken during the Total Solar Eclipse, Aug. 7, 1869. Comm. Ashe.
Some further Observations on *η Argûs* and the surrounding Nebula. Mr. Abbott.
On an early Telescope made by Giuseppe Campani of Rome. Mr. Williams.
On the determination whether the Corona is a Solar or a Terrestrial Phenomenon. Mr. Seabroke.
On *Jupiter's* Satellites. Mr. Severn.
Occultation of *Saturn*, April 19, 1870. Mr. Joynson.
Ditto ditto ditto Mr. Talmage.
Ditto ditto ditto Capt. Noble.
On the Resolvability of Star-Groups regarded as a test of Distance. Mr. Proctor.
On the Proper Motion of *Groombridge* 1830. Mr. Lynn.
Comparison of the Places of the *Tabulæ Red.* and the Radcliffe Catalogue, 1860. Dr. Wolfers.
Note on the above paper. Mr. Stone.
On Meteorological Observations at Gibraltar. Lieut. Brown.
Observations of Occultations of *Saturn*, &c. Mr. Airy.
On *η Argûs*. Mr. Severn.
Loi du Mouvement de Rotation des Planètes. M. Flammarion.
Note respecting the Corona. Prof. Secchi.
Observations at Colebyfield. Mr. Penrose.

June 10. On the Orbit of *α Centauri*. Mr. E. B. Powell.

Note on Father Secchi's Letter to the Academy of Sciences, April 25, 1870. Mr. Seabroke.

Note respecting Solar Spots visible to the Naked Eye. Mr. Hill.

Extract of Letter to Mr. Hind respecting a New Comet. M. Winnecke.

On a Spectroscope in which the Minimum Angle of Deviation and the Prisms are automatically adjusted for the particular Ray of the Spectrum under Examination. Mr. Browning.

On the Alteration in the Colour of the Belts of *Jupiter*. Mr. Browning.

Observations of Winnecke's Comet. Rev. S. J. Perry.

Nov. 11. Further Observations on the Currents of the Solar Photosphere. Mr. Weston.

On the Aurora Borealis of Sept. 24, 1870. Admiral Ommanney.

Observations of Coggia's Comet. Rev. S. J. Perry.

Observations of the Occultation of *Saturn* by the Moon. Mr. Prince.

The Laws of Star Grouping. Mr. Proctor.

On the Use of Screens in Telescopic and other Researches. Mr. Proctor.

On a Contrivance for Extending the Principle of Mr. Browning's Automatic Spectroscope to a Second Battery of Prisms. Mr. Proctor.

On a Standard or Uniform Measure of Time. Col. Drayson.

On a Photograph of *Jupiter*. Mr. Browning.

On the Spectrum of the Aurora Borealis. Mr. Browning.

On a Point in Egyptian Chronology. Mr. Abbe.

On the Automatic Spectroscope for Dr. Huggins. Mr. Grubb.

On the Performance of a Zenith Sector constructed for the Great Trigonometrical Survey, India. Lieut.-Col. Strange.

On Periodical Changes in the Physical Condition of *Jupiter*. Mr. Ranyard.

Dec. 9. On the Proper Motions of 36 A *Ophiuchi* and 30 *Scorpii*. Mr. Lynn.

On the Proper Motion of Oeltzen's Argelander 17415–6. Mr. Lynn.

Observations of Coggia's Comet. Mr. Joynson.

On the Graphical Construction of a Solar Eclipse. Prof. Cayley.

Observations of Solar Eclipse. Baron de Rottenburgh.

1871.

Jan. 16. Summary of Sun-spot Observations made at Kew during 1870. Messrs. De La Rue, Stewart, and Loewy.

Solar Eclipse,	Dec. 22, 1870.	Mr. Joynson.
Ditto	ditto	Mr. Dancer.
Ditto	ditto	Rev. Dr. Robinson.
Ditto	ditto	Mr. Prince.

On a presumed new variable star in *Orion*. Rev. T. W. Webb.

Spots in *Plato*. Mr. Birt.

Solar Eclipse,	Dec. 22, 1870.	Mr. Talmage.
Ditto	ditto	Lieut. Brown.
Ditto	Dec. 11, 1871.	Mr. Ragoonatha Chary.
Ditto	Dec. 22, 1870.	Prof. C. P. Smyth.
Ditto	ditto	Rev. S. J. Perry.
Ditto	ditto	Mr. Ranyard.
Ditto	ditto	Mr. Plummer.

On the change of colour in the Equatorial Belt of *Jupiter*. Mr. Browning.

Work done at the Kew Observatory. Mr. De La Rue.

Solar Eclipse,	Dec. 22, 1870.	Lord Lindsay.
Ditto	ditto	Mr. Stainer.
Ditto	ditto	Mr. Abbay.
Ditto	ditto	Mr. Weston.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
Her Majesty's Government in Victoria.
The Lords Commissioners of the Admiralty.
Her Majesty's Secretary of State for India.
Royal Society of London.
Royal Society, Edinburgh.
Royal Asiatic Society.
Royal Irish Academy.
Royal Geographical Society.
Royal Institution.
Royal United Service Institution.
Geological Society.
Photographic Society.
Society of Arts.
British Meteorological Society.
British Association.
Art-Union of London.
Institute of Actuaries.
British Horological Institute.
The Zoological Society.
Literary and Philosophical Society, Leeds.

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Observatory, Christiania.
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Observatory, Prague.
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United States Naval Observatory.
United States Government.
United States Naval Department.
L'Académie des Sciences de l'Institut de France.
Imperial Academy of Sciences, Vienna.
Imperial Academy of Sciences, St. Petersburg.
Royal Academy of Sciences, Berlin.
Royal Academy of Sciences, Göttingen.
Royal Academy of Sciences, Munich.
Royal Academy of Sciences, Brussels.
Royal Academy Turin.
Royal Institute of Lombardy.
Royal Institute, Palermo.
Imperial Society, Cherbourg.
Academy of Sciences, Batavia.
Physical Society, Berlin.
Royal Society, Copenhagen.
Astronomische Gesellschaft, Leipsig.
Royal Society, Leipsig.
Academy of Sciences, Bologna.
University of Kasan.
American Philosophical Society.
American Association.
American Academy of Natural Sciences.
Smithsonian Institution.
Franklin Institute.
Canadian Institute.
Editor of the Athenæum.
Editor of the Student.
Editor of the Quarterly Journal of Science.

Editors of Silliman's Journal.
Editor of Nature.
Editor of the Electric Telegraph.
Editor of Scientific Opinion.
Editor of the English Mechanic.
Editor of Cosmos.

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Dr. R. Wolf.
Prof. C. A. Young.

The President, after the conclusion of the reading of the Report of the Council, rose to offer a few observations to the meeting. He said:—

In making the announcement that your Council have not this year been able to recommend the usual award of a Gold Medal, I feel confident you will not attribute the cause to their being unable to find any worthy recipient of it. If that had been the case some ground would have been afforded for the apprehension that the pursuit of the science of astronomy was becoming languid, or followed with less ability. I am happy to say, however, that the cause is a very different one, suggesting rather congratulation than regret.

The mutual dependence of the sciences upon each other has been often remarked, and astronomy has, especially of late, been materially aided in its progress by votaries of other sciences. Thus, some discoveries in astronomy of late years, have scarcely been altogether due to the purely astronomical labourer; and we have had instances in which two or more observers in different departments of science have united in the pursuit of a common object. In these joint labours it is difficult to apportion degrees of merit; and your Council, desirous to recognise to the full this collateral aid, have found it impossible on this occasion to select any individual so pre-eminently distinguished by his own independent researches, that they could recommend the Society to bestow its reward upon him, without danger of doing injustice to others.

This difficulty has repeatedly suggested to your Council the desire to bestow a *joint medal;* but, as the bye-laws of the Society do not appear distinctly to contemplate or sanction such a proceeding, the question has arisen, whether the Society might not on a fitting occasion take into consideration whether or not it would be desirable to introduce any modification of the law relative to the annual award of their Medal.

There is another subject on which, if I may be permitted, I would desire to say a few words.

It seems still to be a matter of doubt in the minds of some astronomers, how many satellites ought to be appropriated to the planet *Uranus.* I venture to think that this is a point worthy of determination with all possible certainty; especially as it may be supposed that with the telescopes we now possess, the requisite observations would neither be very protracted nor extremely difficult.

The long delay which has prevented the completion of the mounting of Mr. Newall's great refractor is much to be regretted; as that telescope may be presumed to be especially fitted, and fully competent for the solution of this problem. The planet has already begun to descend in Northern Declination, and the apparent (or projected) orbits of the satellites to close; though,

in both respects, its position is still highly favourable, and the satellites may be seen in every part of their orbits.

The additional testimony of some competent observer, furnished with a telescope sufficient for the work, would be, I cannot help thinking, worth the very moderate amount of time and labour it would require.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXI. *March* 10, 1871. No. 5.

William Lassell, Esq., President, in the Chair.

F. W. Levander, Esq., University College, London;
Wm. Mann, Esq., Royal Observatory, Cape of Good Hope; and
E. W. Snell, Esq., Kidbrook House Academy, Blackheath;

were balloted for and duly elected Fellows of the Society.

On the Total Eclipse of the Sun, on December the 11*th*, 1871, *as visible in the Madras Presidency.* By C. Ragoonathachary, First Assistant, Madras Observatory.

(*Communicated by N. R. Pogson, Esq., Government Astronomer.*)

Herewith I have the honour to submit to the Royal Astronomical Society the results of my calculations with reference to the Total Eclipse of the Sun, which will take place on the 11th December, 1871. Though the duration of this eclipse will be considerably shorter than that of 1868, yet I presume that so favourable an opportunity will not be suffered to pass away without adequate preparation for due record of all the important and interesting phenomena which present themselves for investigation on such occasions.

The central line of the eclipse will first meet the Earth's surface in the Arabian Sea, and entering on the western coast of India, will pass right across one of the most important parts of

Hindustan, in a S.E. by E. direction. In this part of the Peninsula the Sun will be about 20° above the horizon when totally obscured. The duration of totality will be two minutes and a quarter, and the breadth of the shadow about seventy miles. On leaving the eastern coast of the Madras Presidency, the central line will cross Palk's Straits, passing about ten miles S.W. of the island Jaffnapatam, and over the northern part of Ceylon, where the small towns of Moeletivoe and Kokelay will lie near the central line; and also the well-known naval station of Trincomalee, which will be about fifteen miles S.W. of the line. Continuing its course over the Bay of Bengal, the shadow will cross the S.E. point of Sumatra, and will touch the south-western coast of Java, where Batavia, the capital, will lie nearly sixty miles N.E. of the central line; and two other smaller towns, Chidamar and Nagara, will also be very near the middle of the shadow path. In the Admiralty Gulf, on the N.W. coast of Australia, the eclipsed Sun will be only ten degrees past the meridian, and not far from the zenith; in consequence of which the totality will last $4^m\ 18^s$, or only four seconds less than the time of greatest duration. Lastly, passing through the most barren and uninhabited portion of Australia, crossing the Gulf of Carpentaria and the York Peninsula, the shadow will ultimately leave the Earth's surface in the Pacific Ocean.

The following are the geographical positions of the central and limiting lines of the shadow, together with other details of calculation, applicable to Southern India, for intervals of fifteen seconds of Greenwich mean time. They are almost identical with the values obtained by interpolation, from the similar table furnished on page 441 of the *Nautical Almanac* for the year 1871.

Greenwich M.T.	Northern Limit. North Latitude.	Northern Limit. East Longitude.	Central Line. North Latitude.	Central Line. East Longitude.	Southern Limit. North Latitude.	Southern Limit. East Longitude.
h m s	° ′	° ′	° ′	° ′	° ′	° ′
14 25 45	13 1	74 58	12 36	74 42	12 12	74 24
14 26 0	12 48	75 22	12 23	75 6	11 59	74 48
14 26 15	12 35	75 46	12 10	75 30	11 46	75 12
14 26 30	12 23	76 9	11 58	75 53	11 33	75 35
14 26 45	12 11	76 31	11 46	76 15	11 21	75 57
14 27 0	11 59	76 53	11 34	76 37	11 9	76 19
14 27 15	11 47	77 14	11 22	76 58	10 58	76 40
14 27 30	11 36	77 35	11 11	77 18	10 46	77 0
14 27 45	11 25	77 55	11 0	77 38	10 35	77 20
14 28 0	11 14	78 14	10 49	77 57	10 24	77 39
14 28 15	11 4	78 33	10 38	78 16	10 13	77 58
14 28 30	10 53	78 51	10 28	78 34	10 3	78 16
14 28 45	10 43	79 9	10 18	78 52	9 52	78 34
14 29 0	10 33	79 27	10 8	79 10	9 42	78 51
14 29 15	10 23	79 44	9 58	79 27	9 32	79 8

On the Central Line.

Greenwich M.T.	Sun's Altitude above the Horizon.	Excess of the Sun's App. Diameter above that of the Earth.	Relative Motion of Sun in one minute of Time.	Duration of Totality.
h m s	°	"	"	m s
14 25 45	17	65·6	31·0	2 7
14 26 0	17	65·9	30·9	2 8
14 26 15	17	66·2	30·8	2 9
14 26 30	18	66·5	30·7	2 10
14 26 45	18	66·7	30·7	2 11
14 27 0	19	66·9	30·6	2 11
14 27 15	19	67·1	30·5	2 12
14 27 30	19	67·3	30·4	2 13
14 27 45	20	67·5	30·3	2 14
14 28 0	20	67·7	30·2	2 14
14 28 15	20	67·9	30·1	2 15
14 28 30	21	68·1	30·1	2 16
14 28 45	21	68·3	30·0	2 17
14 29 0	21	68·5	29·9	2 17
14 29 15	22	68·7	29·8	2 18

The principal places in the Madras Presidency, situated near the northern limit of the shadow, and their direct distances therefrom in miles, will be as follows:—

Districts.	Places.	Miles.
South Canara	Mangalore	11 within
,,	Oopin Ungadi	upon
Coorg	Mercara	9 within
Mysore	Honsoor	upon
Astragam Division	Mysore	13 beyond
Coimbatore	Sattimangulum	14 within
,,	Bowani	4 within
,,	Yirodu	8 within
Salem	Trichungode	upon
,,	Salem	24 beyond
,,	Namcul	upon
Trichinopoly	Moosery	7 within
,,	Trichinopoly	8 within
Tanjore	Tanjore	5 beyond
,,	Puttoocattay	10 within
,,	Point Calmere	upon

Places most favourably situated on or near the central

line, with their geographical positions and direct distances therefrom:—

Districts.	Places.	North Latitude.	East Longitude.	Miles.
		° ′	° ′	
South Canara	Kassergode	12 30	75 1	5 N
,,	Baicull	12 24	75 4	upon
Coorg	Veerajunderpetta	12 13	75 52	16 N
Malabar	Gunote	12 0	75 45	2 S
,,	Manuntoddy	11 48	76 5	1 S
,,	Goodaloor	11 30	76 32	5 S
On the Neelgherries	Ootacamund	11 25	76 43	6 S
	Dodabetta	11 23	76 47	5 S
	Wellington	11 23	76 46	6 S
	Coonoor	11 21	76 52	5 S
	Kotagherry	11 24	76 56	2 N
Coimbatore	Sivamogay	11 20	77 5	2 N
,,	Avenasi	11 12	77 19	2 N
,,	Tirrupur	11 5	77 24	2 S
,,	Kangyam	11 1	77 37	upon
,,	Darapoorum	10 44	77 35	18 S
,,	Vellacoil	10 57	77 46	2 S
,,	Chinna Darapoorum	10 51	77 55	1 N
,,	Caroor	10 57	78 8	14 N
Madura	Veerallimalli	10 34	78 37	10 N
,,	Iluppur	10 31	78 41	5 N
,,	Poodoocottah	10 23	78 53	5 N
Tanjore	Ardangi	10 11	79 3	1 S
,,	Manamalgudi	10 3	79 16	2 S

And, lastly, for places near the southern limit of the shadow we shall have,—

Districts.	Places.	Miles.
Malabar	Cannanore	14 within
,,	Tellicherry	10 within
,,	Mahe	9 within
,,	Calicut	10 beyond
,,	Beyapur	15 beyond
,,	Palghaut	10 beyond
Coimbatore	Coimbatore	14 within
,,	Polachy	4 beyond
,,	Chuckragherry	8 beyond
Madura	Pulney	upon
,,	Dindigul	11 within
,,	Madura	9 beyond

Districts.	Places.	Miles.
Madura	Shevagunga	1 beyond
,,	Ramnaud	20 beyond
,,	Autencurray	16 beyond
,,	Ramaswarum	8 beyond

The calculation of the different phenomena of the eclipse was made accurately for Avenasi, a railway station situated midway between the two coasts, on the central line; and for the Madras and Trevandrum Observatories, which lie respectively at some distance north and south of the shadow. The usual equations of reduction applicable to places near the above three points are also given; in which l denotes geocentric north latitude, λ_Δ east longitude for Madras, and t the Madras mean time of each phenomenon. The results of the calculations are:—

For Madras.

Lat. $13^\circ\ 4'\cdot 1$ N.; Long. $80^\circ\ 14'\cdot 3$ E. of Greenwich.

		Madras Mean Time.
		d h m s
Time of first contact		Dec. 11 18 47 37
Time of greatest obscuration		19 49 32
Time of last contact		20 59 59
Duration of the eclipse		2 12 22
Angle from north point, of	First contact	73° 33′ West.
	Last contact	119 9 East.
Angle from the Sun's vertex, of	First contact	1 19 Right.
	Last contact	173 33 Left.
Magnitude of the eclipse (Sun's diameter = 1)		0·9165
Limb of the Sun eclipsed		South.

Formulæ for Reduction to different places near Madras.

First Contact.

$$\text{Cos } w = -0\cdot2214 - [0\cdot20128] \sin l - [9\cdot98190] \cos l \cos (\lambda_\Delta - 121^\circ\ 15'\cdot 5)$$

$$t = 21^h\ 35^m\ 36^s - [3\cdot55274] \sin w - [3\cdot35619] \sin l$$
$$- [3\cdot79315] \cos l \cos (\lambda_\Delta + 6^\circ\ 24'\cdot 3)$$

Greatest Phase.

$$\text{Cos } w = -0\cdot0382 - [0\cdot19815] \sin l - [9\cdot99342] \cos l \cos (\lambda_\Delta - 107^\circ\ 48'\cdot 3)$$

$$t = 21^h\ 45^m\ 3^s - [3\cdot42904] \sin l - [3\cdot84182] \cos l \cos (\lambda_\Delta + 20^\circ\ 47'\cdot 6)$$

$$\text{Magnitude of the eclipse} = 1\cdot0175 - (1\cdot0175 \cos w)$$

Last Contact.

$$\text{Cos } w = 0\cdot2244 - [0\cdot19578] \sin l - [0\cdot00173] \cos l \cos (\lambda_\Delta - 92^\circ\ 3'\cdot 4)$$

$$t = 21^h\ 38^m\ 9^s + [3\cdot65898] \sin w - [3\cdot49952] \sin l$$
$$- [3\cdot89599] \cos l \cos (\lambda_\Delta + 37^\circ\ 3'\cdot 3)$$

For Avenasi.

Lat. 11° 12′ N.; Long. $\begin{cases} 77° \ 19'.0 \text{ E. of Greenwich.} \\ 2 \ \ 55.3 \text{ W. of Madras.} \end{cases}$

		Madras Mean Time. d h m s		Local Mean Time. h m s
Time of first contact ..	..	Dec. 11 18 47 33	=	18 35 52
Beginning of the total phase	..	19 47 19	=	19 35 38
Middle of totality ..	..	19 48 26	=	19 36 44
Ending of the total phase	..	19 49 32	=	19 37 51
Time of last contact ..	..	20 57 31	=	20 45 50

			h m s
Duration of the Eclipse	..	..	2 9 58
Duration of totality ..	..	..	0 2 13

		° ′	
Angle from north point, of	First contact	68 8	West.
	Last contact	113 26	East.
Angle from the Sun's vertex,	First contact	7 3	Left.
	Last contact	171 44	Left.

Formulæ for Reduction to different places near Avenasi.

First Contact.

$$\text{Cos } w = -0.2075 - [0.20204] \sin l - [9.97944] \cos l \cos (\lambda_\Delta - 120° \ 51'.1)$$

$$t = 21^h \ 32^m \ 10^s - [3.54328] \sin w - [3.34120] \sin l - [3.78431] \cos l \cos (\lambda_\Delta + 6° \ 32'.3)$$

Middle of Totality.

$$\text{Cos } w = -1.9887 - [1.96825] \sin l - [1.76185] \cos l \cos (\lambda_\Delta - 107° \ 36'.0)$$

$$t = 21^h \ 42^m \ 37^s - [3.41653] \sin l - [3.83247] \cos l \cos (\lambda_\Delta + 20° \ 51'.7)$$

$$\text{Semi-duration of totality} = [1.82258] \sin w.$$

Last Contact.

$$\text{Cos } w = 0.2277 - [0.19709] \sin l - [9.99844] \cos l \cos (\lambda_\Delta - 91° \ 31'.4)$$

$$t = 21^h \ 37^m \ 24^s + [3.65178] \sin w - [3.48527] \sin l - [3.88985] \cos l \cos (\lambda_\Delta + 37° \ 14'.3)$$

For Trevandrum.

Lat. 8° 30′.5 N.; Long. $\begin{cases} 76° \ 59'.8 \text{ E. of Greenwich.} \\ 3 \ \ 14.5 \text{ W. of Madras.} \end{cases}$

		Madras Mean Time. d h m s		Local Mean Time. h m s
Time of first contact	..	Dec. 11 18 48 37	=	18 35 39
Time of greatest obscuration ..		19 49 30	=	19 36 32
Time of last contact	..	20 58 21	=	20 45 23

			h m s
Duration of the eclipse	..	..	2 9 44

Angle from north point, of	First contact	63° 29′ West.
	Last contact	108 56 East.
Angle from the Sun's vertex, of	First contact	14 15 Left.
	Last contact	170 4 Left.
Magnitude of the eclipse (Sun's diameter = 1)		0·9371
Limb of the Sun eclipsed		North.

Formulæ for Reduction to different places near Trevandrum.

First Contact.

$$\text{Cos } w = -0{\cdot}2099 - [0{\cdot}20201] \sin l - [9{\cdot}97990] \cos l \cos (\lambda_{\Delta} - 120^{\circ}\ 55'{\cdot}0)$$

$$t = 21^{h}\ 31^{m}\ 48^{s} - [3{\cdot}54223] \sin w - [3{\cdot}34107] \sin l - [3{\cdot}78326] \cos l \cos (\lambda_{\Delta} + 6^{\circ}\ 31'{\cdot}2)$$

Greatest Phase.

$$\text{Cos } w = -0{\cdot}0358 - [0{\cdot}19843] \sin l - [9{\cdot}99265] \cos l \cos (\lambda_{\Delta} - 107^{\circ}\ 40'{\cdot}8)$$

$$t = 21^{h}\ 42^{m}\ 22^{s} - [3{\cdot}41661] \sin l - [3{\cdot}83136] \cos l \cos (\lambda_{\Delta} + 20^{\circ}\ 50'{\cdot}1)$$

$$\text{Magnitude of the eclipse} = 1{\cdot}0173 - (1{\cdot}0173 \cos w)$$

Last Contact.

$$\text{Cos } w = 0{\cdot}2263 - [0{\cdot}19676] \sin l - [9{\cdot}99962] \cos l \cos (\lambda_{\Delta} - 91^{\circ}\ 42'{\cdot}0)$$

$$t = 21^{h}\ 37^{m}\ 26^{s} + [3{\cdot}65169] \sin w - [3{\cdot}48752] \sin l - [3{\cdot}88948] \cos l \cos (\lambda_{\Delta} + 37^{\circ}\ 10'{\cdot}8)$$

The approximate details of the eclipse for Baicull on the western coast, Ardangi near the east coast, and Trincomalee in Ceylon, all of which will be near the central line, will be as follows:—

For Baicull.

Lat. 12° 24′ N.; Long. { 75° 4′·0 E. of Greenwich. / 5 10·3 W. of Madras.

	Madras Mean Time. d h m s		Local Mean Time. h m s
Time of first contact ..	Dec. 11 18 47 5	=	18 26 23
Beginning of the total phase ..	19 45 51	=	19 25 9
Middle of totality	19 46 55	=	19 26 13
Ending of the total phase ..	19 47 59	=	19 27 17
Time of last contact ..	20 54 13	=	20 33 31

	h m s
Duration of the Eclipse ..	2 7 8
Duration of totality ..	0 2 8

Angle from north point, of	First contact	69° West.
	Last contact	113 East.
Angle from the Sun's vertex, of	First contact	6 Left.
	Last contact	171 Left.

For Ardangi.

Lat. 10° 11′ N.; Long. $\begin{cases} 79° \ 3'.0 \text{ E. of Greenwich.} \\ 1 \ \ 11.3 \text{ W. of Madras.} \end{cases}$

		Madras Mean time. d h m s		Local Mean Time. h m s
Time of first contact	..	Dec. 11 18 48 7	=	18 43 22
Beginning of the total phase	..	19 48 49	=	19 44 4
Middle of totality	..	19 49 57	=	19 45 12
Ending of the total phase	..	19 51 5	=	19 46 20
Time of last contact	..	21 0 15	=	20 55 30
Duration of the eclipse	..	h m s 2 12 8		
Duration of totality	..	0 2 16		
Angle from north point, of	First contact	68° West.		
	Last contact	114 East.		
Angle from the Sun's vertex, of	First contact	8 Left.		
	Last contact	171 Left.		

For Trincomalee.

Lat. 8° 33′ N.; Long. $\begin{cases} 81° \ 24'.0 \text{ E. of Greenwich.} \\ 1 \ \ 9.7 \text{ E. of Madras.} \end{cases}$

		Madras Mean Time. d h m s		Local Mean Time. h m s
Time of first contact	..	Dec. 11 18 49 9	=	18 53 48
Beginning of the total phase	..	19 51 19	=	19 55 58
Middle of Totality	..	19 52 16	=	19 56 55
Ending of the total phase	..	19 53 13	=	19 57 52
Time of last contact	..	21 4 23	=	21 9 2
Duration of the eclipse	..	h m s 2 15 14		
Duration of totality	..	0 1 54		
Angle from north point, of,	First contact	67° West.		
	Last contact	114 East.		
Angle from the Sun's vertex, of	First contact	9 Left.		
	Last contact	170 Left.		

I have adhered throughout to the method of Mr. Woolhouse, adopting the positions of the Sun and Moon as given in the *Nautical Almanac.* Subjoined are the approximate positions of bright stars and planets most conspicuous to the naked eye during the time of totality, referred to the zenith of Avenasi.

		Zenith Distance. °	Azimuth °			Zenith Distance. °	Azimuth °
1	Castor	78	N 59 W	17	β Centauri	72	S 8 E
2	Procyon	80	N 86 W	♀	Venus	28	S 39 E
3	Pollux	76	N 63 W	19	Arcturus	20	N 60 E
4	Jupiter	71	N 71 W	20	α² Centauri	74	S 12 E
5	α Hydræ	58	S 72 W	21	ι Bootis	29	N 52 E
6	Regulus	44	N 84 W	22	α Libræ	38	S 45 E
7	γ¹ Leonis	41	N 71 W	23	β Urs. Min.	66	N 8 E
8	η Argus	76	S 18 W	24	β Libræ	39	S 59 E
9	α Urs. Maj.	56	N 16 W	25	α Cor. Bor.	38	N 62 E
10	δ Leonis	29	N 66 W	26	α Serpentis	40	S 87 E
11	β Leonis	19	N 76 W	27	β Scorpii	54	S 56 E
12	γ Urs. Maj.	45	N 14 W	28	Antares	62	S 52 E
13	α¹ Crucis	74	S 5 W	29	α Herculis	61	N 81 E
14	Polaris	80	0	⊙	Sun	71	S 61 E
15	Spica	23	S 13 E	31	α Ophiuchi	66	N 80 E
16	η Urs. Maj.	40	N 11 E	32	γ Draconis	71	N 39 E

To facilitate the independent determination of the longitude of any place of observation, I have calculated such occultations of stars by the Moon as will occur about a week before or after the day of the eclipse. It unfortunately happens, however, that during this time only one bright star lies within the limits of the Moon's path, and so I have been obliged to rest contented with much smaller stars than are usually selected for such a purpose. The computations have been made for Madras and Avenasi, by an approximate method, which usually gives the times within a minute, and the angular points of contact within a degree, of those found by a more refined process. The times at Avenasi will differ but slightly from those for any other spot along the shadow line, in its course across India.

Occultations as seen at Madras.

Date.	Stars.		Mag.	Disappearance. Madras Mean T.	Angle from N. Point.	Angle from Vertex.	Reappearance. Madras Mean T.	Angle from N. Point.	Angle from Vertex.
1871.				h m	°	°	h m	°	°
Dec. 3	η Leonis		$3\frac{1}{2}$	14 35	128 E	143 Right	16 8	84 W	9 Right
6	XII. 394 Weisse		9	14 13	147 E	136 Right	15 9	92 W	15 Right
14	20114	Oeltezon Argelander's zone.	9	6 41	16 E	50 Right	7 5	44 W	115 Right
14	20133		9	7 1	53 E	16 Right	7 43	80 W	154 Right
14	20138		$8\frac{1}{2}$	7 7	109 E	39 Left	7 45	136 W	150 Left
15	21093		8	8 22	83 E	11 Left	9 16	128 W	156 Left
16	21791		8	6 9	36 E	8 Right	7 26	100 W	160 Right
16	21810		$7\frac{1}{2}$	6 31	88 E	37 Left	7 36	150 W	148 Left

Occultations as seen at Avenasi.

Date.	Stars.		Mag.	Disappearance. Madras Mean T.	Angle from N. Point.	Angle from Vertex.	Reappearance. Madras Mean T.	Angle from N. Point.	Angle from Vertex.
Dec. 3	η Leonis		$3\frac{1}{2}$	14 32	136 E	137 Right	15 58	89 W	15 Right
6	XII. 394 Weisse		9	14 16	155 E	125 Right	15 5	103 W	24 Right
14	20114	Oeltez n Argelander's zone.	9	6 38	18 E	48 Right	7 7	46 W	117 Right
14	20133		9	6 59	55 E	15 Right	7 43	82 W	157 Right
14	20138		$8\frac{1}{2}$	7 6	110 E	40 Left	7 45	138 W	147 Left
15	21093		8	8 21	84 E	12 Left	9 15	132 W	151 Left
16	21791		8	6 2	36 E	8 Right	7 15	100 W	160 Right
16	21810		$7\frac{1}{2}$	6 26	87 E	37 Left	7 32	151 W	148 Left

The general circumstances under which the Total Eclipse of Dec. 11th, 1871, will occur, are singularly and unusually favourable, the greater portion of the shadow-path being easily accessible by means of the railway and good public roads; while a well-managed line of telegraph will afford facilities for that most incomparable means of fixing the longitude of the place of observation with regard to Madras. The favourite Sanitarium of the Presidency, Ootacamund, will doubtless be selected by many persons as a convenient and familiar station from which to observe the eclipse; as also the hilly region of Wynaad, in the Malabar district, where numerous European gentlemen reside for the purpose of superintending their coffee-plantations. The lofty peak of Dodabetta, the highest point of the Neilgherries, 8640 feet above sea-level, would agreeably to the often-repeated and enlightened view of Prof. C. Piazzi Smyth, the Astronomer Royal for Scotland, offer a grand opportunity for spectroscopic observations, in an atmosphere of small density and free from all the impurities which abound at lower levels, but unfortunately haze and mist are very prevalent on the hill-ranges in the month of December. The weather is in general fine elsewhere about that time along the shadow-path, but more especially so eastward of the Neilgherry hills than towards the Malabar coast.

Madras,
5th December, 1870.

Erratum in Results of Greenwich Observations of Solar Eclipse of 1860, *July* 18.

(*Communicated by the Astronomer Royal.*)

On examining the reduction of these Observations it was found that, owing to the printing of 1 L for 2 L in the Third Series of Observations, the wrong signs were given to the coefficients of $d\,S$ and $d\,s$; the following corrections are therefore required in the Volume of *Greenwich Observations* for 1860:—

Page 71, Solar Eclipse, Signs of Coefficients of $d\,S$ and $d\,s$ *for* − *read* +.

Page 77, Equations given by Transits of Limbs, Signs of Coefficients of $d\,S$ *for* − *read* + and of $d\,s$ *for* + *read* −.

Page 78, in the Third Final Equation the Sign of the Coefficient for $d\,S$ *should be* + and for $d\,s$ −.

The correct solution of the Equations is as follows:—

$$
\begin{aligned}
d\,a &= -\ 35''\!\cdot\!816 \\
d\,S &= -\ 2\cdot235 \\
d\,s &= -\ 3\cdot350 \\
d\,\delta - d\,\Delta &= -\ 8\cdot349
\end{aligned}
$$

Correction to Moon's Tabular Longitude:—

$$
\begin{aligned}
&\textit{for} - 36''\!\cdot\!94\ \textit{read} - 34''\!\cdot\!32 \\
\text{E. N.P.D. } &\textit{for} - 1\cdot77\ \textit{read} - 1\cdot85
\end{aligned}
$$

In the paper by the Astronomer Royal, in vol. xxi. of the *Monthly Notices*, on page 156, line 15,

for "Sun's 1 L & Moon's 1 L"
read "Sun's 2 L & Moon's 2 L"

Page 157,	line 3,	*for*	− 38·63	*read*	− 35·816
,,	,, 4,	,,	− 8·75	,,	− 8·349
,,	,, 5,	,,	− 1·45	,,	− 2·235
,,	,, 6,	,,	− 2·97	,,	− 3·350
From bottom	,, 4,	,,	− 1·1	,,	+ 1·68
,, ,,	,, 3,	,,	− 4·0	,,	− 3·55
,, ,,	,, 2,	,,	+ 0·3	,,	− 0·54
,, ,,	,, 1,	,,	− 2·4	,,	− 2·75

Observations of the Solar Eclipse of 1870, *Dec.* 21–22, *made at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

As this eclipse afforded a very favourable opportunity of determining the error of Hansen's Lunar Tables at the period of conjunction, a plan of observations, similar to that arranged for the eclipse of 1860, was prepared by the Astronomer Royal, and carried out as far as the weather permitted.

With the Great Equatoreal four quantities were to be determined, viz.:—

da =	Correction to excess of Moon's R.A.			over Sun's R.A.		
$d\delta - d\Delta$ =	,,	,,	,,	N.P.D.	,,	N.P.D.
dS =	,,	Sun's Semidiameter				
ds =	,,	Moon's ,,				

for which the following arrangement of observations was made:—

(1) A series of differences of N.P.D. of the Upper and Lower Cusps, showing principally the effect of the correction,

$$ds + dS + da.$$

(2) A series of differences of times of transit of Moon's 2 L. and Sun's 2 L., giving

$$ds - dS + da.$$

(3) A series of differences of N.P.D. of Moon's N.L. and Sun's N.L., giving

$$-ds + dS + (d\delta - d\Delta).$$

(4) A series of differences of N.P.D. of the Right and Left Cusps, giving chiefly the effect of da.

(5) The same as series (3).

(6) A series of differences of times of transit of Sun's 1 L. and Moon's 1 L., giving

$$-ds + dS + da.$$

(7) A series of differences of N.P.D. of the Upper and Lower Cusps, giving

$$ds + dS - da.$$

The first set having been lost through cloud, the remainder furnished five resulting equations, formed by taking the sums of the equations, corresponding to each series of observations, obtained by a comparison of the measured with the tabular quantity as affected by the symbolical corrections given above. These being treated by the method of least squares gave four equations, of which the following is the solution:—

$$
\begin{aligned}
da &= -6''.47 \\
d\delta - d\Delta &= +1.21 \\
dS &= -1.68 \\
ds &= -0.49
\end{aligned}
$$

From meridian observations on Dec. 20, 23, 24, 28, 29, the Sun's tabular error was found:—

$$
\begin{aligned}
&\text{In R.A.} &&= +0^{s}.11 \\
&\text{In N.P.D.} &&= +2''.2
\end{aligned}
$$

Combining this result with that given above, we obtain as the tabular error of the Moon:—

$$
\begin{aligned}
&\text{In R.A.} &&= +0^{s}.54 \\
&\text{In N.P.D.} &&= +1''.0
\end{aligned}
$$

The effect of these corrections on the choice of stations for observing the totality appears to be very small. The central line would be shifted nearly 5 miles further south, and the breadth of the shadow increased by about double that amount, whilst the time of first contact would be accelerated and of last contact retarded by about 10^{s}.

The altazimuth observations give error of tabular altazimuth:—

Of Moon's 2 L.	$= -11''.00$	from	6 Obs.
,, 1 L.	$= -3.83$	,,	12 ,,

Whence error of Tab. Geoc. Semidiam. $= +3''.48$.

Applying this as a correction to the semidiameter used in the reduction of the observations of Z.D. of Moon's U.L. we find tabular error of Moon:—

$$
\begin{aligned}
&\text{In R.A.} &&= +0^{s}.54 \\
&\text{In N.P.D.} &&= +2''.54
\end{aligned}
$$

As from altazimuth observations of the *bright* Moon the error of semidiameter in azimuth $= -0''.5$, the eclipse observations give for the sum of irradiations of Sun and Moon with the altazimuth $4''.0$ for an object-glass of $3\frac{1}{4}$-inches.

With the Great Equatoreal (aperture $12\frac{1}{4}$-inches) the value appears to be about $0''.5$.

The Solar Eclipse, Dec. 22, 1870. By the Rev. S. J. Perry.

(*Communicated by W. H. H. Hudson, Esq.*)

A mere glance at the published results of the late Eclipse Expeditions will be sufficient to convince any one, who is at all conversant with the subject, of the great advance that has just been made in our knowledge of solar physics. That so much should have been accomplished, when the obstacles to success

seemed to meet the observers at every turn, is a matter of no little surprise, and the value of the observations already published is an earnest of what we are to expect from a careful discussion and arrangement of the collective results.

No one will doubt but that much good work has been done during the two short minutes of totality by the polariscope and spectroscope, by the telescope and eye observations, and by photography, and it would be a matter of no small difficulty to determine at present the relative merits of these several aids to our observers. We shall therefore form a fair idea of what has been effected by the various modes of observation, if we pass in rapid review the triumphs of a single one. Let us take spectroscopy, for instance.

Passing over the accurate determination of the time of first contact, of which a warning of 15ˢ was given by Prof. Young, from the observed gradual extinction of the bright lines of the chromosphere by the Moon's dark limb, we come to the observations that have a more direct bearing on solar physics.

If we start from the photosphere in an outward journey, we find at the outset that the observations of Prof. Young and Mr. Pye of London have satisfactorily closed a discussion which has occupied so many pages of the *Comptes Rendus*, the *Philosophical Magazine*, and other publications during the last twelve months; as to the existence of a thin absorption shell dividing the photosphere from the chromosphere. What had so far been invisible to our English spectroscopists, but what Secchi, favoured by a clear Roman atmosphere, had seen as a continuous spectrum, burst forth during the eclipse as an innumerable mass of bright lines apparently identical in position with the dark absorption lines of the solar spectrum. This glorious sight lasted but for one or two seconds, being visible only when the stronger light of the photosphere was eclipsed, and eclipsed itself in turn by the dark Moon as soon as visible. This thin shell of vapours, scarce 1000 miles in thickness, possesses an importance far greater than its relative extent alone would justify, if we may look upon it as the seat of that selective absorption which gives existence to the dark Fraunhofer lines.

Continuing our outward journey we come next to the chromosphere, whose constitution and wondrous forms, and still more wondrous motions, are now a matter for daily study and observation, and therefore possess less of interest for the observer than those other spherical shells of vapour, which can only be examined during the fleeting moments of totality. The addition of some new faint line, or of the exact form of a few solar prominences, could scarcely add appreciably to the knowledge acquired by the labours of Lockyer, Young, Respighi, and others, and would not repay the loss of time so valuable for other researches.

But the outer layer of cooler hydrogen, whose tale has just been told by the spectroscope of Mr. Abbay, is more worthy of present attention. The bright lines 1474 and F were seen

together; F less brilliant than 1474. Mr. Pye, by whom a similar observation was made of 1474, C, D_3, and F, estimated the relative brightness of the four lines, and found them to vary as the numbers 100, 85, 55, and 30. The observations of Denza and Carpmael, who both saw two lines in the corona, and those of Harkness, Barton, &c., will probably be found to confirm the above; and may, moreover, give the definite portion of the corona under observation, which Abbay and Pye could not do from the nature of their instruments.

Hydrogen, therefore, which gives a spectrum so much more bright than the green line 1474 when seen in chromosphere, must in the observations under discussion be far less heated, and becoming thus unable to send us in any strength its crimson light, shines mainly with a borrowed solar light. Hence the strong radial polarization; hence the silvery white of the leucosphere, or ring-formed inner corona, which I suppose must consist in great part of the cooler hydrogen, mixed of course with the green vapour of the line 1474. Burton's observation of the extension of the hydrogen lines also tends to strengthen this position, and to establish the reality of the cooler hydrogen envelope.

The absence of the dark Fraunhofer lines should be of little weight, considering the faintness of the spectrum.

But the argument from the varying brilliancy of the lines appears somewhat weakened by the observation of Capt. Maclear, who found the hydrogen lines everywhere,—in prominences, on corona, on dark Moon,—their varying brightness depending not on their internal heat, but on the mode of their reaching us, either as direct or dispersed rays. This strong dispersion of the chromospheric lines by cloud and atmosphere also renders any accurate determination of the limits of the several solar envelopes, by aid of the spectroscope, a matter of the greatest difficulty.

But whatever the exact extent of the inner shells of vapour, of the absorbing envelope, or chromosphere, or leucosphere, we come at last to a hollow sphere of vapour lighter than hydrogen, whose spectrum is the bright green 1474, whose internal diameter we may perhaps roughly guess at, but which outwardly stretches forth into space, probably far beyond any limits we are likely accurately to measure. Prof. Young followed this line, as it gradually diminished in brightness, from the photosphere to a distance of 16′ from the Moon's limb, whilst Winlock traced it to 20′; but the bright lines seen on the dark Moon tell us that the observed extent may in part be due to the dispersion from the atmosphere. The observations of Young and Winlock are confirmed by those of Burton, Harkness, Pye, Abbay, and others, thus leaving no room for us to doubt the existence of this vast sphere of vapours, which will probably enter largely into all future theories of the ether of space.

On the Colour of the Moon during the late Eclipse.
By Richard A. Proctor, B.A. (Cambridge).

During the late eclipse the Moon's disk appeared green. It was compared by one observer to dark green velvet. At first sight it might appear as though a full explanation of this was supplied by the fact that the air towards the Moon's body was illuminated by the corona, the principal line in whose spectrum is green. But as the corona itself did not appear green, we must suppose that the chief portion of the corona's light was in reality that which gave the faint continuous spectrum; and this must needs be the case with the coronal light reflected by our own atmosphere. It may be assumed, therefore, that the coronal (reflected) light received from the direction of the Moon's body could not have been appreciably green; and the observed greenness of the Moon's disk must be otherwise explained.

It seems to me that a sufficient explanation is to be found in the nature of the light received by the Moon from the Earth during the Eclipse. This light as respects quantity must have been considerable,— in fact (for equal surfaces) some thirteen times that with which the full Moon illuminates the Earth.* Its colour must have been green, I think; because the proportion of land and sea surface in the terrestrial disk, as seen from the Moon, was such that calling the ocean blue-green and the land brownish (on the average), the resulting mixed colour would be a deep green. (In the *Quarterly Journal of Science*, for October last, I have shown the exact orthographic presentation of the Earth's disk towards the Moon near the epoch of totality.)

Now during the Eclipse of 1860 land and sea were turned towards the Moon in different proportion. The eclipse occurred in summer, so that the northern or land portion of the Earth was less foreshortened, and furthermore the eclipse occurred at about three in the afternoon, by which hour the two Americas were well advanced upon the Earth's disk as seen from the Moon. One would, therefore, expect that the Moon's disk on that occasion should have presented a brown hue. And accordingly we find Mr De La Rue so describing it.

It would appear probable, therefore, that our Earth, as seen from distant stations, as *Venus* or *Mercury*, is usually a green planet, but sometimes dun or fawn-coloured. Also her rotation may probably be recognised without telescopic aid, simply by her colour-changes.†

* The Moon's shadow on the Earth would have the effect of diminishing this light by the same amount as if, instead of umbra and penumbra, there were a black shadow whose foreshortened aspect seen from the Moon equalled the Moon's disk as seen by ourselves.

† When this paper was read, another explanation of the Moon's colour was suggested; and, further, the reality of the colour was called in question. But I would note that the explanation I here offer, whether sufficient or not, undoubtedly refers to a *vera causa*. We know how bright a distant hill illuminated by the full Moon appears; now the Moon's surface is certainly illuminated thirteen times more brightly by the "full" Earth, during total eclipse. Nor can it well be questioned that the earth-light must be at times strongly coloured.

Note on the Corona. By Richard A. Proctor, B.A.

A year since, I had occasion to address the Society on the subject of the corona. I then pointed out that no direct sunlight can illuminate the part of our atmosphere which lies towards and around the Moon's place; and the purpose with which I wrote was fulfilled when the views I dealt with were withdrawn from public attention. At that time it seemed to me, on the one hand, very unsafe to theorize about the actual constitution of the corona; but, on the other hand, it seemed demonstrable that the corona is a solar appendage. At present we are, I think, more favourably circumstanced; more especially on account of those photographic successes with which astronomers—as well workers as thinkers—have such good reason to be satisfied.

I propose to place before the April meeting of the Society certain views to which, as I think, the recent observations seem to point. At present, however, my object is merely to note what I take to be by far the most important contribution to our knowledge respecting the corona. I cannot regard the differentiation of the corona into two portions* as an acquisition, simply because for more than a century and a half the distinction had been recognised by astronomers. Nor can I attach any signal importance to the proof afforded by recent observations that the corona has a great extension from the Sun; nor to the confirmation of the long-disputed American observations; because both these points were in effect established before the eclipse took place.

The great result of the recent eclipse observations will be found, unless I mistake, to lie in the association now shown to exist between the configuration of the inner and brighter portion of the corona and its outer and more strikingly radiated portion. This is shown unmistakably by a comparison of the accounts of Lieut. Brown and Prof. Watson with the photographs of Mr. Willard and Mr. Brothers. It was indeed strikingly evidenced during the eclipse of 1869, but not absolutely demonstrated. It seems to me a fact of the utmost importance and significance; more especially when combined with the seemingly established relation between the regions of greatest prominence-disturbance and the expansions of the inner part of the corona, and with that other relation which associates the spot-zones with the larger and more active prominences.

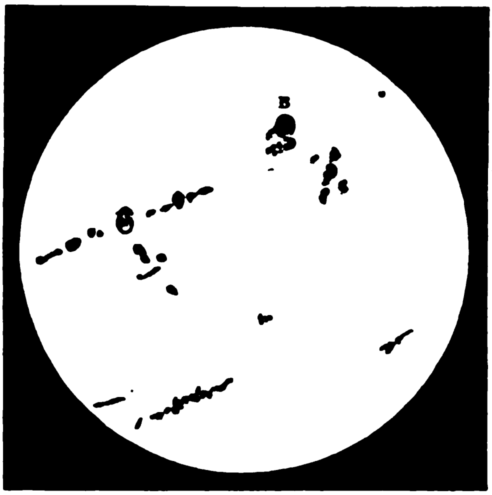
It might seem at first sight that the long radiations opposite the bright parts of the inner corona could be explained as due to the illumination of our own atmosphere in corresponding directions. But the simple consideration of the way in which our atmosphere is illuminated by the corona will show that no radia-

* I would submit that the word "defined" applied in the Report of the Council to the outline of the inner part of the corona, requires to be modified or explained; for undoubtedly the inner corona was not bounded by what is usually understood as a defined outline. The border of this part of the corona showed a rapid but not sudden degradation of brilliancy, having a perfectly soft outline nowhere sharply defined.

tions extending outwards could make their appearance without corresponding expansion on either side, and a yet more marked extension inwards over the Moon's disk.* The uniformity of the light over the Moon's disk, its faintness, and especially its observed inferiority to that received 15′ from the Moon's limb, would suffice to disprove the imagined explanation, which is also, however, opposed to the simplest optical considerations.

I conceive we have now clear evidence of a form of action—but whether eruptive, electrical, or simply repulsive, is not as yet obvious—exerted outwards to enormous distances by the Sun, and with maximum energy over the spot-zones, but local, variable, and probably intermittent.

The annexed woodcut of the spots on the Sun about the time of the Eclipse of December last, should have accompanied Lieut. A. B. Brown's account of the Eclipse as given in the January number of the *Monthly Notices.*



Sun-Spots, Dec. 18, 1870 (12.15 P.M.)

* It was with much pleasure that I heard Mr. Brothers read at the last meeting a letter from Dr. Balfour Stewart, in which views were expressed precisely similar to those above enunciated. In the same letter Dr. Stewart pointed out (as I had shown in the *Monthly Notices* for March, 1870), that though our atmosphere towards the Moon's place is undoubtedly illuminated by light from the prominences, sierra and inner corona, yet that the quantity of light received in this way can bear no higher proportion to the actual light of the corona and prominences than the atmospheric glare in full sunlight bears to such sunlight. It has been this argument — absolutely demonstrative, despite its extreme simplicity—which has caused me for many months past to feel complete certainty respecting the general nature of the corona.

Report of Professor W. G. Adams, on Observations of the Eclipse of December 22nd, 1870, made at Augusta, in Sicily.

King's College, London, February, 1871.

Before the final arrangements were made by the Organising Committee I was asked to join the English Eclipse Expedition. Not being able to leave with the other Members of either Expedition, on account of my lectures at King's College, I followed the Sicilian party on the evening of Friday, the 9th of December, and joined them on the morning of Wednesday, the 14th, in Naples. The *Psyche* arrived during the day, and we went on board, and she steamed out of harbour at four o'clock. It was a peaceful afternoon, with no wind and a perfectly calm sea, and the view of the bay, with Naples in the sunshine and Vesuvius sending out light puffs of steam and smoke, was very beautiful. The sea kept perfectly calm through the night, and on the morning of the 15th, after passing Messina, a meeting was held and observers were stationed. The instruments were brought on deck, and we were enjoying the scenery of the coast and of Etna when the unfortunate wreck of the *Psyche* cast a gloom over our Expedition and entirely altered our arrangements. It was intended that Syracuse should be the head-quarters, but now Mr. Lockyer took up his station at Catania. Professor Roscoe took charge of a party to observe on the slopes of Etna, and I was asked to take charge of the Augusta Expedition. The other observers for Augusta were —

Mr. Brett, with a reflector for sketching the corona;

Mr. Burton, with a spectroscope;

Mr. Clifford, Mr. Ranyard } for observations on polarization;

Mr. Samuelson, for general observations.

Colonel Porter also volunteered to sketch the corona, and some officers of the Royal Engineers were expected from Malta for the day of the eclipse.

Mr. Brett was asked to proceed at once to Augusta and arrange with Colonel Porter about our camp and observatory.

With such an excellent practising ground as the terraces and gardens of the Benedictine Monastery, it was thought better that the Etna and Augusta observers should stay at Catania until Monday, the 19th, and practise with their instruments. As there had been no opportunity for testing the instruments, this comparison was very important, especially for all those who were to observe for polarization; some of whom had never seen their instruments. All the observers for polarization except Mr. Ranyard remained, and arranged the sets of observations to be made by each observer, so that if the weather was favourable, the determination of the polarization of the corona should be complete. We found this previous practice of very great value, and before we parted Mr. Griffiths and I made comparative observations with our polarimeters with very satisfactory results. Turning to

the same points of clouds and buildings, we determined the plane of polarization, and measured the degree of polarization in the light at the rate of from 10 to 15 seconds for each point of observation; and in some cases our readings both for plane and degree of polarization were absolutely identical.

Description of Instruments.

To my own telescope (2¼-inch object-glass), an altitude and azimuth instrument, I had fitted at the principal focus of the erecting eye-piece a plate of right-handed and left-handed quartz, a small Nicol's prism being placed at the stop. I had also requested Mr. Ladd to fit to my telescope a polarimeter eye-piece, consisting of a Savart's polariscope (a plate of quartz cut obliquely, and a Nicol's prism), with four plates of glass for a depolariser. Just before I left for Sicily, Professor Stokes expressed a wish that I would use the double-rotating quartz to test the observations made by Prazmowski at the Eclipse of 1860, and recommended that Mr. Ranyard and I should observe at different places along the central line. Accordingly, it was arranged with Mr. Ranyard at Catania, that on the morning of the 22nd he and another of our party should drive some miles up from Augusta, in the direction of the hills of Carlentini to observe the eclipse.

On comparing my two eye-pieces, I found that when the polarization was not strong, the double-rotating quartz eye-piece was not nearly so delicate as the polarimeter eye-piece for detecting the plane of polarization, but gave a clearer view of the point of observation. In addition to these, Mr. Becker had fitted up for me another polarimeter to be used without a telescope, consisting of four glass plates, a very delicate natural quartz crystal, giving right-handed and left-handed rotation, and a Nicol's prism. When the planes of polarization of the polariser and analyser are coincident, the crystal gives a central white band with a tinge of red on the outside of it; then a dark purple band, gradually shading outward through blue, green, and yellow, to the pink field covering the remainder of the crystal. On turning the analyser the bands move parallel to themselves across the field, each colour occupying the position of its complementary colour, when the analyser has been turned through a right-angle, the tint on the outside of the field being a bluish green. The crystal was placed behind the glass plates and the Nicol's prism at the other end of a telescopic tube. By drawing out the tube to about one foot in length the field of view could be reduced to about six diameters of the Moon. This tube was supported and could turn in another tube which was attached to a wooden stand which allowed of motion in altitude and azimuth. By position circles on the fixed tube and on the box containing the glass plates, the plane and degree of polarization could respectively be determined. The existence of the dark bands in this crystal made it so delicate that, with faint light

when there was not a trace of colour to be seen, I could detect polarization and approximately determine the plane, and on a dark day I could determine the plane and the degree of polarization on the clouds when there was not more than one per cent of polarization. With this instrument I had intended to make observations on the sky before the eclipse, and during totality to observe the streamers and the body of the Moon if time permitted, in addition to making two or three observations on the Corona with the Prazmowski arrangement. I had hoped to have an officer of the Royal Engineers to assist me and confirm my observations, and should have taught him how to use the instrument for the observations during totality. I had intended to register the observations by drawing lines against the index as a ruler with a pencil of a different colour for each observation, writing the number of the observation with the same pencil. I also took out with me an extra telescope, the eye-piece being fitted with a plate made up of two wedges of quartz with their axes at right-angles to one another and to the axis of the tube. Mr. Pierce, jun., of the American party, was at Catania, and as his spectroscope had not arrived, the eye-piece of this telescope was lent to him to fit to his own small telescope, Mr. Ranyard having taken its proper tube and object-glass to Augusta.

On Monday, the 19th, Mr. Clifford and I left Catania in a boat to go to Augusta. In consequence of a breeze from the sea we did not reach Capo San Croce until eight o'clock, and after turning it we had a strong land breeze against us from the west. Twice during the evening, about half-past six and again about seven o'clock, we saw a brilliant display of the aurora. It was far more brilliant than the remarkable aurora which I had seen in October, and more beautiful than any I had ever seen. First, pink streamers shot up perpendicularly to the eastern horizon, passing across the planet *Jupiter*. I judged that these must have been in or very near to the ecliptic. These were followed by other streamers shooting up from the north and north-east until the sky became covered with a pinkish mauve colour. Round the eastern and northern horizon there was a faint hazy grey light as if a thick mist were coming on, but which was seen to be connected with the aurora by its fitfulness. The zodiacal light was seen in the western sky near the ecliptic as a hazy white light. As the evening grew darker we saw the strong phosphorescence of the sea. There was a bright glow on the drops thrown up by the oar as it struck the water, and the forms of the eddies caused by the bending of the oar were distinct and brilliantly illuminated.

Through the kindness of Prof. Cavalieri Cacciatore of the Palermo Observatory, and Father Denza, who made comparisons for time, I was able to obtain the latitude and longitude, as well as the local times of the different phases of the eclipse according to their calculations.

Latitude of Fort Augusta	.. ..	37° 14′ 0″·6 N.
Longitude (in time)	.. ..	$1^h\ 0^m\ 52^s{\cdot}2$ E.
Difference of longitude between Augusta and Palermo	.. ..	$7^m\ 26^s{\cdot}6$

Local Times.

		Italian. h m s	Hind. h m s
First contact ..	..	0 39 17	
Beginning of totality	..	2 1 56·7	2 1 51·8
Middle.. ..	..	2 2 52·1	2 2 46
End of totality ..	..	2 3 47·5	2 3 40·3
End of eclipse ..	..	3 21 21	
Duration of totality	..	1 50·8	1 48·5

The following observations were made with my Aneroid Barometer registering from 31 inches to 15 inches, and which had been regularly compared with a standard barometer, and found to indicate pressures very accurately.

Day.	Hour.	Pressure.	Remarks.
Dec. 20	9 A.M.	29·87	
	12 NOON	29·82	
	3 P.M.	29·82	
	6 P.M.	29·77	
21	7 A.M.	29·68	
	12 NOON	29·65	
	8 P.M.	29·50	Report of bad weather in Italy and Sicily; wind westerly; sirocco expected.
	11 P.M.	29·45	
22	7 A.M.	29·28	Dec. 22, 2 A.M. Sirocco from the west; very heavy rain; frequent thunder and lightning. 6 A.M. No rain; little wind; lightning in eastern horizon and in heavy stratus cloud to the south-east. 7 A.M. Rain and heavy clouds. 7 15 A.M. Wind rising and becoming violent; clouds in patches.
	11 45 A.M.	29·37	
	12 10 P.M.	29·32	Sky of a watery light blue.
	12 40 P.M.	29·28	Time of first contact.
	3 P.M.	29·30	
	4 40 P.M.	29·30	Wind again becoming violent.
	6 P.M.	29·35	Very high wind.
23	8 30 A.M.	29·44	Bright morning, followed by a fine day.

My barometer was compared with Mr. Burton's smaller aneroid, which showed greater variations than mine, but generally in the same direction.

The five divisons of our observatory were occupied in order by:—

(1) Mr. Brett.
(2) Mr. Burton.
(3) Colonel Porter.
(4) Mr. Clifford.
(5) Myself.

Each observer had one of the Sappers to assist him in preparing his instruments and in recording any notes which it might be necessary to make during totality. Where they were required, tables formed by driving piles into the ground and nailing planks on them were erected for our telescopes.

On the 21st we were able to get our instruments into position, and the Sappers showed their skill in repairing parts of instruments which were out of order.

On the 21st, at 2 P.M., the hour of totality, I fixed my telescope with one foot raised above the level of the table, so that when the telescope was pointed to the Sun, the single motion of rotation about the pillar of the telescope (*i.e.* the motion for change of azimuth), would keep the Sun in the field of view, by making the telescope describe a tangent to the Sun's path. I found it necessary to tilt the pillar of the telescope through an angle of 15° for this purpose.

I considered that besides the convenience of having only one motion, there would be an advantage in referring my observations for polarization to known diameters other than the vertical and horizontal diameters.

The black line shows the position of the line of division of my crystal in the observations which I had planned. By moving my telescope in azimuth in one direction or the other, I passed into my second or third positions, passing through the highest point or the lowest point of the Moon's limb respectively.

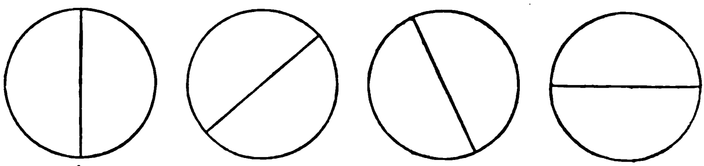
The constants of my telescope are as follows:—

Aperture of object-glass	2¼ inches.
Focal length	31 inches.
Diameter of field	About 50′.
Magnifying power	25.

so that the Moon's apparent diameter was about two-thirds of the diameter of the field. The Nicol's prism was so placed that for a beam of polarised light the two sides of the crystal were of the same pink tint in the centre when the index was at zero, the line of division of the crystal being then in the plane of polarization. On turning the eye-piece, the light on the right changes towards the more refrangible end of the spectrum, and the light on the left becomes red, then purple, and blue, until when it is turned through a right angle both sides at the centre are of the same uniform green tint. Turning from the first

position in the opposite direction, the changes take place in the opposite direction.

With Prazmowski's arrangement, I had to bring the Sun into the centre of the field, and to observe first the tint on the two sides at the junction where the line of division of the two quartz plates cuts the limb, then to observe and note the changes of colour round the limb of the Moon on both sides. Then to pass to my second position, and to make the same series of observations, then to pass to my third position, and again observe for each point in the same way. If time remained, after these



1 2 3 4

observations had been satisfactorily made,—as I had no one to assist me who could make the observations,—I should have used my delicate polarimeter, which was on its stand close to the telescope, and already directed on the Moon, to determine the plane and degree of polarization of the streamers. It was arranged that about two minutes before totality one of the Sappers should give the time at intervals of ten seconds, and that at the last minute before totality he should begin to count continuously and give every five seconds until after totality.

Large thick clouds were flying from the west, and frequently obscuring the Sun and rapidly passing away. When the Sun was about half eclipsed, there was rather a sudden change in the light and a sudden chill was felt in our observatory. About three minutes before totality there were brilliant and very remarkable patches of red and yellow light on a cloud to the right and rather below the Sun. This cloud passed away, and the Sun was quite clear. The time of totality was now very near, and a cloud from the west was threatening to blight our hopes; the band of sunlight was getting exceedingly thin and seemed to be breaking up into sections, when at the very instant the dense cloud came over the Moon and shut out the whole, so that it was doubtful whether the Moon or the clouds first eclipsed the Sun. For a full minute I could detect nothing of the Moon's disk, but moved my telescope very gradually, then the cloud became thinner, and I found the Moon still in the centre of the field. At the top and bottom I saw the dark limb, but no light outside it at these my first points of observation. To the north-east I saw light of the corona or prominences covering some 20° of the limb, and also a fainter light on the western limb of the Moon. I could not detect any colour or any difference of colour denoting polarization on the two plates of my bi-quartz. The Moon was

again obscured entirely, but after a few seconds I again detected light of the corona near the point of emergence, and placed the line of division of my bi-quartz radial to the Moon, having the light in the centre of the field, but I could detect no trace of colour on the two parts of the crystal. The view then became continuous, and the totality was over.

I had found in my previous trials that for faint polarization the bi-quartz was not nearly so delicate as the quartz wedge or the Savart's bands, and these observations show that the polarization, either of the cloud or the corona, was so slight, that it was beyond the power of the bi-quartz crystal to detect it.

I had a very strong impression left on my mind that during totality, the light, which was of greater intensity than I had expected, was different from ordinary white light, and from many experiments I am strongly of opinion that bands rather than colour should be trusted to detect delicate polarization.

Mr. Clifford observed light polarized on the cloud to the right and left and over the Moon, in a horizontal plane through the Moon's centre, and found the plane of polarization inclined at from 15° to 20° to the vertical. At his last observation on the Moon, when it was visible, the plane of polarization was vertical.

Mr. Clifford also saw the corona near the point of emergence about the end of totality.

It will be seen from Mr. Clifford's observations that the plane of polarization by the cloud was very nearly parallel to the line of division of my crystal, *i.e.* nearly at right angles to the motion of the Sun.

Mr. Burton indicated the positions of some of the most remarkable prominences during the morning of the 22nd, and just before totality saw several lines in the spectrum (between D and E) in the chromosphere. At the beginning of totality, with his slit tangential and as near as possible to the east limb, a bright line was distinctly seen in the spectrum, very near E, and a little less refrangible. The line was less defined than the hydrogen lines of the prominences. On account of cloud, this was the only observation of the spectrum which Mr. Burton was able to make. No dark lines were seen. I examined the spectrum with the instrument after the eclipse was over, and found that with the width of the slit used the definition of the dark lines was exceedingly good. Mr. Burton had also intended to observe in order the west, north, and south points. Mr. Burton saw the corona on west side at the end of totality for about two seconds with the naked eye. He says, "The colour was *silvery white* fading *gradually* in brilliancy outward. I am not certain whether the outer boundary was definite or not, I incline to the latter opinion."

Table to Determine the Degree of Polarization of Light refracted through Four Parallel Plates. By Prof. W. G. Adams.

If $\frac{p}{n+p}$ represent the proportion of polarized light in the refracted beam, then, employing the formulæ given in the *Philosophical Magazine* for March, we have

$$\frac{n+p}{p} = \frac{\frac{1}{\sin^2(\phi - \phi_1)} + \frac{7}{\sin^2(\phi + \phi_1)} - 4}{4}$$

where ϕ and ϕ_1 are the angles of incidence and refraction respectively.

For $\mu = 1{\cdot}5$ (crown glass), Angle of complete polarization 56° 18′ 36″
For $\mu = 1{\cdot}513$ Angle of complete polarization 56° 32′

From the results for these two values of μ the values for $\mu = 1{\cdot}54$ are deduced, and for any value of μ the degree of polarized light may readily be calculated from the formula. The values as given in the Paper referred to, are,—

ϕ	10°	15°	20°	21° 30′	25°	30°	35°	40°
$\frac{p}{n+p}=$	·0107	·02443	·04430	·05683	·07111	·10498	·14745	·19845
For $\mu = 1{\cdot}513$								
$\frac{p}{n+p}=$	·01103	·02504	·04540	(·05813)	·07267	·1076	·1509	·2029
Probable values for $\mu = 1{\cdot}54$								
$\frac{p}{n+p}=$	·0116	·0263	·0476	..	·076	·113	·158	·212

ϕ	45	50°	55°	56° 18′ 36″	60°	65°	70°	72°
$\frac{p}{n+p}=$	·25786	·32383	·39231	·40984	·4560	·50530	·5309	·53305
For $\mu = 1{\cdot}513$				(For 56° 32′)				
$\frac{p}{n+p}=$	·2635	·3305	·3999	·4204	·4642	·5139	·5394	(·5416)
Probably values for $\mu = 1{\cdot}54$								
$\frac{p}{n+p}=$	·275	·344	·415	..	·481	·531	·556	

From the table, for the values of $\frac{p}{n+p}$ we see that for four plates the proportion of polarized light in the beam which passes through the plate still goes on increasing when the incidence becomes greater than the angle for complete polarization, and that at about 72° it attains its greatest value, when there is about 53 per cent of the light polarized.

I have also formed a table for $\mu = 1.513$, of which the results are given; and from these we can derive close approximate values for the proportions of polarized light for values of μ not differing much from these values.

The probable values for $\mu = 1.54$ are given in the table.

If we employ the four plates as a depolarizer to determine the proportion of polarized light in the incident beam by reducing the light to its ordinary unpolarized state, then the proportion of polarized light in the incident beam will be given, for any angle of complete depolarization, by the value of $\frac{p}{n+p}$ for that angle.

Extract from a Report of the Eclipse Expedition of 1870, *at Augusta, Sicily.* By John Brett, Esq.

During the violent squalls of the night of the 21st, some heavy rain fell, and the morning of the Eclipse dawned badly. A sketch was made of the sunrise, which was red and threatening; but after daylight the sky partially cleared, and all the morning we remained in suspense, hoping perhaps, but dreading certainly.

Two members of our party were now detailed to Villa'smunda, and a Sapper, named Buchanan, was taught by Professor W. G. Adams to call the time in the observatory.

As the day advanced the sky rather improved, and the Sun's limb was fairly steady. The apparatus in my compartment of the observatory consisted chiefly of a reflecting telescope of silvered glass, 8½ inches aperture and 7 feet focus, made by Mr. Browning, with an altitude and azimuth mounting of extraordinary steadiness. The defining power of this instrument is of first-rate excellence; and one side of the tube being entirely removed, no air-currents are generated in it by the Sun's heat, nor are any musical vibrations set up by the wind. Affixed close to the eye-tube was a card with a black disk on it for the purposes of drawing, and another for writing notes. The observations were commenced with a solar eye-piece, consisting of a first reflection prism, to which were applied alternately a Kellner with a field of 45′, power 75; a Kellner with field of 35′, power 100; and one of Mr. Browning's exquisite achromatic doublets, power 200; the two former supplied with dark glass, the latter with a sliding wedge. The first contact was beautifully seen and the exact time noted, the limb being fairly steady and the instrument perfectly so. Ten minutes later I suspected traces of a weak halo round the Sun, and at twenty minutes after first contact this had become more pronounced, presenting a tolerably well-defined boundary at a distance of about 2′ from the limb. I begged Mr. Burton, my next neighbour in the observatory, to bring his experienced eye to bear upon it, which he did. At the same time he called my attention to a tinge of redness towards the Sun's limb (on the disk), of the

colour of iron rust, which remained permanent. The halo was of a pallid grey colour. It was equally distinct under either of the eye-pieces mentioned above, and at a later stage of the eclipse the Moon's disk was decidedly visible beyond that of the Sun for about 2′ projected with a clear edge upon the halo. I could come to no other conclusion than that this was really the corona; yet it seemed to me a suspicious circumstance that it did not grow much more vivid as the eclipse approached totality. The Moon's limb was beautifully defined on the Sun's disk, and *considerably sharper* than the limb of the Sun. The contrast of the two limbs in this respect was very remarkable. There was, however, a weak illumination encroaching upon the Moon, of a greyish tint, but it faded away from the limb inwards imperceptibly, and was nowhere nearly so intense as the halo or corona of the Sun. Occasionally some clouds passed over, but nothing in any degree disturbed or interfered with the halo.

At Mr. Burton's suggestion I very carefully observed the eclipse of two large spots with power 200 for distortion, but found no trace of any, definition being then good. After this period I directed my attention chiefly to the cusps; but, although they were blunted more or less, nothing remarkable occurred until five or six minutes before the total phase, except that I satisfied myself that the Moon's disk was only visible off that of the Sun, as far as the halo or corona extended. The wind which had been strong all day now increased considerably; some of the more violent gusts being very inconvenient in the observatory; and for instruments not very steadily mounted would have been a serious hindrance. There were plenty of clouds about, but not threatening total obscuration, and hopes of a good observation were entertained freely. The silence within the observatory was contrasted in a singular manner by the increasing roar and howling of the populace without, who seemed to have assembled in a dense multitude about the further borders of the glacis in a state of great excitement. The time seemed immensely long. I was keeping my attention on the cusps, and had already seen one or two beads nipped off by the Moon's mountains, and the time had come to prepare for the total phase. About five minutes before that period I went to the side of the observatory and looked over at the landscape for a few seconds, but beyond a general gloom over all, of a leaden lurid hue such as I expected, I saw nothing particularly remarkable.* The clouds were getting worse, and the approaching shadow was not visible on the hills.

I returned to the telescope without delay, removed the solar eye-piece and introduced the Kellner, power 100, with dark glass,

* I believe that the wonderful character of the terrestrial appearances recorded during eclipses is to a great extent the measure of the observer's ignorance of ordinary cloud phenomena. Such appearances (to judge from my own experience) would hardly strike a landscape-painter as very abnormal; but the truth is, that very few persons besides artists have ever paid enough attention to clouds and their effects upon landscape scenery to enable them to answer the most elementary questions respecting them.

to take a last look at the cusps before introducing the large field eye-piece with which I proposed to examine the corona. I now beheld a beautiful sight: a delicate and exquisitely defined little ray or cone of light emanating from the south cusp in a direction tangential to the Moon's limb.

Its length was not more than 3′; possibly something less. Its light was less intense than that of the cusp, but as beautifully defined as anything I ever saw in my life. Its termination at the weak end or base of the cone was clearly visible and not diffused indefinitely, and the angle at the apex was small, not more than 5°.

In my drawing of this phenomenon I have rather exaggerated the angle and also the diffuseness of the base, in order to show its character, for the refinement of drawing necessary to represent accurately so delicate an image is forbidden by the coarseness of the materials employed. Having examined this for a few seconds, I brought the north cusp into the field and found a similar ray there, but somewhat smaller. The cloud over the Sun now became so dense as to impede vision seriously, and cause the most profound anxiety. The beads were being nipped off the cusp in rapid succession, and presently the whole rim broke up instantly into fragments of various lengths, and disappeared, and left utter darkness. The dark glass (which I held in my hand) was discarded, but not a trace of anything was visible. I quickly pulled out the eye-piece and replaced it with the lowest power, for the agonising thought occurred to me that the object would presently be out of the field and lost beyond hope; for my finders could do nothing towards penetrating the cloud. I quickly slid in the large field eye-tube but still saw nothing. It might now, I thought, have passed even out of this field, but I dared not move the instrument. The counting of seconds by the Sapper had already advanced to a frightful number, and these were periods of misery never to be forgotten.

Near at hand I had set up a small but beautiful achromatic telescope to supplement my large instrument, fitted with a terrestrial eye-piece that gave a field of a degree and a half and power of 20, hoping by it to be able to take in the streamers (if there were any) at one view. I now turned to that; when presently the prominences burst all at once into view; and I immediately went back to the reflector and happily got a magnificent sight of them; but I was astonished and disappointed at not finding any distinct boundary to the corona, nor any hint whatever of streamers. The whole limb was literally crowded with prominences, varying in colour from white and yellow to pink and nearly crimson. One crooked flame-coloured one was particularly conspicuous.

The appearance on the whole was far more like terrestrial conflagration than I expected.* One respect in which this

* Especially the combustion of wood in a baker's oven when nearly exhausted.

resemblance seemed to me obvious was the proportion both in quantity and luminousness which the flames bore to the haze of shapeless vapour in which they were mixed up. This haze was so like what one sees in a terrestrial conflagration that the idea never would have occurred to me that it was self-luminous. The light that showed no defined form was not a clear flat halo, but suggested the uncertain turmoil of the results of combustion in a furnace.

Some of the tongues or prominences were extremely vivid, but others were much mixed up with the general light surrounding the limb. There were no spaces where you could say this is mere corona, for at a greater distance than the apex of the highest flame I can hardly say there was any decided light. I don't think there was anything visible beyond 5′ of the limb, and the highest flame might have been probably 4′. I must confess I was extremely vexed not to see anything like the corona I expected; I should never have called this a corona at all. The basis of it was a seething mass of flames of a languid character; some of them so languid as to lose the spiral shape of flame and lie in heaps, and the intervals between them were apparently filled with gaseous material which might be illuminated by them. I do not think the disk was ever entirely free from cloud during totality. There were certainly no streamers, so I conclude they must have been intercepted by the cloud. I do not mean merely that their light was, but their cause also; for I assume that the shadow of the Moon must inevitably chill and condense much floating moisture in our own air, and send it down as dew. And these falling particles would reflect the light emitted by the prominences, and the effect of perspective would give them an appearance of radiating beams from the Moon's limb, or rather from that neighbourhood, although the regularity of radiation might be interfered with by various terrestrial causes. I conclude that the dew which would have formed streamers was, at Augusta, intercepted or prevented by the cloud; although the latter was not dense enough to conceal much. I have often seen *Jupiter's* belts beautifully through a denser cloud certainly. I did not see *Saturn* on this occasion, it is true. I must confess, however, that I entirely forgot to look for him, my attention being too intently fixed on the subject of the corona.

The view, on the whole, I suppose lasted about ten seconds, but my drawing,* though necessarily done afterwards, gives a very fair general representation of what I saw. I marked down the positions of three chief prominences, and went again to the small telescope to seek for streamers, but found none, and the Sun almost immediately burst forth, compelling me to quit the instrument.

* The drawings referred to in this paper will be published by the Organising Committee with the rest of the Report.

On the Photographs taken at Syracuse during the Eclipse of the Sun, December 22nd, 1870. By Mr. Brothers.

The pre-eminent use of photography for determining points in dispute has seldom perhaps been so plainly seen as in its application to astronomical matters. First of all, in 1860, Dr. De La Rue and Father Secchi settled the question as to the prominences belonging to the Sun; and now, ten years later, it may, I think, be said that photography has disposed of the corona as one of the Sun's appendages.

The variety of drawings which have been published, no two pictures of the same eclipse ever agreeing in all particulars, conclusively showed that to photography must we look if we wished to obtain evidence which should be beyond dispute. The success of Dr. Curtis and Mr. Whipple had proved that more of the corona could be photographed than had previously been shown on the sensitive plate. But much remained to be shown — the eye could see more than had ever been photographed.

In considering the means to be adopted for photographing the eclipse on Dec. 22nd last, the question occurred to me, Why have not the photographic plates shown the corona as it is visible to the naked eye? And the answer appeared to be that the proper kind of instrument had not hitherto been used. An ordinary telescope, whether reflecting or refracting, is not suitable for the purpose. It would be of very little importance whether the image were large or small if only the corona in all its detail could be shown on the collodion plate; and yet the image of the Moon's disk must be of such dimensions that a good enlargement from it could afterwards be made. What was required was a *picture* of the eclipse, and it seemed to me that such a picture could only be taken with an ordinary photographic lens; one of long focus, and yet capable of giving an image with a moderately short exposure. Through the kindness of Mr. Dallmeyer, one of his "Rapid Rectilinear" lenses was lent me for the purpose of the Expedition, and with that instrument my pictures of the corona were taken. This lens is of 4 inches aperture, 30 inches focus, and gives an image of the Sun of about three-tenths of an inch in diameter. The image bears enlargement to 1½ inches diameter with very good definition, notwithstanding the doubling of the image caused by the high wind which was blowing at the time the photographs were taken.

I have no doubt that good pictures of the corona could be obtained with a lens of still shorter focus, better probably than any hitherto taken; and in proof of this I have here three pictures of the eclipse taken by one of my assistants at Manchester with a lens of 16 inches focus and 4 inches aperture, giving an image ⅙-inch in diameter. The copies now before you are enlarged four diameters and the definition is good.

It is proposed this evening to show on the screen, by means of the lime-light, transparencies of four of the negatives taken at

Syracuse during the totality. The plates were exposed in the following order, and with the results stated:—

		Seconds of Totality.	Results.
Spare time	3		
First exposure	3	3rd to 6th	Blank
Change plate	6		
Second exposure	18	12th to 30th	Fair
Change plate	6		
Third exposure	30	36th to 66th	Slight
Change plate	6		
Fourth exposure	15	72nd to 89th	Fair
Change plate	6		
Fifth exposure	8	95th to 103rd	Good
Spare time about	3		
Total	104		

The sixth plate taken in the telescope and exposed at the 36th second, for 3 seconds, for the prominences, gave no satisfactory result.

It is extremely difficult to describe the differing appearances of the pictures, but attention may be directed to two or three points. It will be noticed that the cloud which at one time threatened to prevent any photographs being taken, has obscured many of the details which are shown in the fifth plate; but the red prominences, shining with light of much greater photographic intensity than the corona, have left their impressions on the plates, and some of them can be seen in all four. No. 2 has the two between which the great rift shows in the fifth photograph, and these two prominences are shown in all four plates; others were covered by the Moon in passing over the Sun's disk. In No. 3 the light of the corona is about equal on the east and west sides, and is scarcely at all visible on the north and south. In Plates 2, 4, and 5, however, it will be noticed that the preponderance of the corona is seen on the westerly side; and as No. 2 shows this effect, it may, I think, be assumed that it was real, and did not depend on the Moon uncovering the westerly side as she passed on her course.

The effect of the cloud in obscuring detail is seen when we compare No. 5 with the other plates. In this plate we notice as the chief feature a great rift or gap, just between two red prominences on the south-west side of the Moon's limb; and there are also gaps distinctly marked on the east and west sides, with indications of similar features in other places. It may, perhaps, be suggested that these rifts are not visible in any of the other plates because the rifts were formed only at about the time the fifth picture was taken. This cannot, however, have been the case, as the great rift is seen in the same relative position on the plate

taken by the American photographers at Cadiz, and which was exposed during 1½ minute, or nearly the whole time of the totality. Now if this phenomenon had not been persistent, and only became visible during the last 20 seconds of totality at Cadiz, it could not have been shown at all, as the picture would have been "burnt out" at this part, and no dark space could have been shown.

The great rift in the corona is more distinctly shown in the original negative than in the transparency, which will be exhibited on the screen. The copy is made by transmitting light through the negative, and the consequence is that much of the finer detail is lost. When, however, the negative is viewed by reflected light it is seen that the corona extends on the north and westerly sides to at least two diameters of the Moon, and on the east and south sides to about one diameter.

When the original negative is viewed by transmitted light it exhibits features which I have endeavoured to show, though very imperfectly, in the copy of a drawing which will be exhibited on the screen. There are some places where there has been a greater intensity of light extending in one place about 10′ from the Moon's limb, and near this part there are other dense portions of varying height, and these rayed portions of the corona are of various forms, some are slightly curved in one direction and some in another. Most of this detail is lost in the process of copying; the general effect only will be seen on the screen. Photography cannot reproduce its own work in this case; and no words can accurately describe what this negative shows. It may perhaps be possible to reproduce some of the detail in a pencil drawing.

I do not venture to offer any opinion as to the cause of the rifts or dark spaces in the corona, but of their coincidence in position when the various drawings and photographs are placed side by side there cannot be the slightest doubt. This I shall endeavour to show on the screen when it will be noticed that Prof. Watson's drawing, and the American and my own photographs exhibit coincidences of a very remarkable character.

The following remarks on the subject, contained in a letter from Sir J. Herschel, will I am sure be considered valuable:—

"Assuredly the decidedly marked notch or bay in both photographs (those taken at Cadiz and Syracuse) agreeing so perfectly in situation (marked so definitely by its occurrence just opposite the middle point between two unmistakable red prominences) is evidence not to be refused of its extra atmospheric origin, and *almost* conclusive of its proximity to the solar globe. I see nothing which gives me the idea of rays or streaky irradiation such as that figure in the Washington Observations for 1870—Plate XII.—which, if it *could* be believed, would point to lunar mountains as the origin of the dark spaces, and bring the whole phenomenon within the distance of the lunar orbit. A terrestrial atmospheric origin is quite out of question."

On the subject of the effect of the outer corona being due to

atmospheric glare, I have received the following letter from Prof. Balfour Stewart, who says:—

"In the case of the uneclipsed Sun we have the well-known luminosity surrounding his disk, caused no doubt by atmospheric glare, so that here we may be said to have sunlight *plus* glare; when, however, the Sun is totally eclipsed, the light which we see is derived not from the body of the Sun himself but from his appendages, and the atmosphere acting in the same way upon this light as it did upon that of the uneclipsed Sun we have a complex phenomenon before us consisting of solar appendages *plus* its atmospheric glare.

"Now I see no reason why the proportion between the real light and the glare should not be the same in both these cases; that is to say, light of the Sun *is to* its glare *as* light of the solar appendages *is to* its glare. Taking your photograph as a trustworthy record of what was visible at the late eclipse, let us endeavour to ascertain how much of the photographic effect is due to atmospheric glare. I think I am right in stating two things with regard to this photograph. In the first place, the details of the luminous appendages surrounding the Sun are well represented. Secondly, there is little or no photographic effect over the centre of the Moon's disk. Judging from the photographs of the Sun himself taken by the Kew heliograph, we may conclude that when those pictures are not overdone, but give the various details of the solar disk, the exposure has been too small to give us the faintest trace of atmospheric glare which surrounds the Sun. Making use of the proportion above stated I should conclude that a photograph of the solar appendages not overdone but presenting, like yours, the details of these appendages, would not present a perceptible trace of their atmospheric glare.

"In the next place, the atmospheric glare due to the light from the eclipsed Sun should be thrown *inwards* over the Moon's disk as much as *outwards* beyond the true corona, the light therefore which reaches us in a total eclipse from the centre of the Moon's disk, and which may be partly due to earth-light reflected from the Moon, may be safely taken as somewhat exceeding that which can possibly be due to atmospheric glare; and inasmuch as in your photograph there is very little effect in the centre of the Moon's disk, I am led to think that very little of the result obtained can be due to glare.

"I have here confined myself strictly to your photographs, but the principle laid down is applicable to all kinds of observations; and I must confess that I cannot at the present moment see why the streamers, if they are caused by the atmosphere, should invariably shoot outwards and never venture to trespass upon the Moon's disk, at any rate this is a point that will bear discussion."

A careful examination of the original negative shows that the amount of deposit over the Moon's disk is equal to that shown at about $1\frac{1}{3}$ diameter from her limb, but this I believe to be due to

chemical deposit, and has nothing whatever to do with the action of light.

When prints from the second and fifth negatives are combined stereoscopically the effect of relief is produced. In the interval of 73 seconds, the Moon has changed her place sufficiently to show that the corona is not an atmospheric phenomenon. If it had not happened that the great rift, occurring between the two prominences shown so distinctly in the American and my own photographs, had decided the solar origin of the corona, these two photographs would probably have been sufficient for the purpose.

On the Zodiacal Light. By A. C. Ranyard, Esq.

The conviction that the accumulation and comparison of evidence on a subject so interesting and yet so little understood as the zodiacal light cannot but be useful, induces me to submit the following observation to the notice of the Astronomical Society:—

On the evening of the 19th December last the detachment of the Eclipse Expedition encamped at Agosta had an opportunity of examining a particularly brilliant display of the zodiacal light, stretching to some 80° from the Sun's place: its contour was of a somewhat conical form, blunted at the apex, the semi-vertical angle of the cone being about 12°. Its light was apparently white, and was undistinguishable in point of colour from the light of the Milky Way, which also shone out with considerable distinctness upon the night in question. I was anxious to see if any polarization could be detected, and for this purpose made use of a Savart, and at first thought that faint lines were visible, indicating polarization in a plane through the Sun; but not being at all sure of my observation, I asked Mr. Burton, who was formerly assistant to Lord Rosse, and has very keen eyesight, to look through the instrument: he at once said that he distinctly saw bands brightest when the Savart was turned so that the direction of the bands passed through the Sun, and that the centre band was black; he also saw the bands perpendicular to the Sun's direction, but could not determine the nature of the centre one. I should mention that the Savart was so set as to give a black centre when the bands were parallel to the plane of polarization.

I then asked Mr. Burton to see if he could detect any bands upon the sky away from the zodiacal light, but he was unable to do so even at 90° away from the Sun's position.

I again took the instrument, but was unable to see any bands myself. On the next night the zodiacal light was again visible, but with considerably less brilliance than before. Father Secchi observed it with a Savart, and thought that he detected faint bands, but said that he could not be sure of his observation.

If other observations should confirm those of Mr. Burton we shall be in possession of proof, not only that the zodiacal light consists of matter which reflects the Sun's light, but that—

(1) That matter exists in particles so small that their diameters are comparable with the wave-lengths of light; or

(2) It consists of matter capable of giving specular reflection.

On a Contrivance for protecting the Observer when a Reflecting-Telescope of large size is used in the open air. By J. Browning, Esq.

It is generally admitted that reflecting-telescopes of large aperture perform most satisfactorily when they are used in the open air; the currents caused by convection, when the instrument is protected, through the slit of the dome of the observatory, being very prejudicial to good definition. But it is, of course, very desirable that the observer should be protected from winds and cold, particularly when measuring or drawing. I have contrived what I prefer to term "an observing-box," which I think will be found useful, inasmuch as it will afford complete protection to the observer under all circumstances.

The diagram which I shall now describe shows the principal details of this contrivance; but I have prepared a carefully executed model of the contrivance, so as to be able to exhibit the action to the members of the Society. In the diagram, A, A, A represent three triangular pillars, which are hollow. These are attached to B and C, two strong triangles, framed of quarterings.

The observing-box D, which is also triangular, fits loosely between the three upright pillars, and runs in grooves, placed on the sides which face inwards. The weight of the observing-box is supported by three ropes, which run over the pillars P, P, P, attached to A, A, A. These are carried down the hollow pillars, and weights, which collectively must be heavier than the box and the observer, are suspended from them. In this manner the weight of the box is counterbalanced. A door in one side of the box enables the observer to enter when the box is near the ground. On the lower ledge of the opening in this door, at a convenient height, a desk slope is placed; this can be lowered on hinges, or raised, and kept in position by a turning bracket in the usual way. The observer sits in an ordinary chair, as it has been found more comfortable than any seat attached to the box. Two strong cords attached to bars running across the triangles A and B, are carried through small circular holes in the top and bottom of the observing-box, one on each side of the observer. To raise or lower himself, the observer has only to take hold of

these, and if his weight is pretty accurately counterbalanced, a very slight exertion of force will enable him to ascend and descend readily to any height he may desire. In the portion of this apparatus which I have completed, I have found that a force of seven pounds was quite sufficient for this purpose. By having the weights, as I have stated, heavier than the box and the observer, and counterbalancing the box by superfluous weights on the inside, the box may be used by any person who is heavier or lighter than the observer for whom it was constructed. Ventilators with gauze wire, and sliding shutters which may be

opened or shut at the will of the observer, are placed on each side of him; as also a Miller's smoke-top, as a ventilator in the roof of the box. In particularly inclement weather, a cloth or India-rubber head may be attached to the framework of the door. This need have only a small circular opening, in which may be sewed a strong elastic band. On springing this over the eye-piece of the telescope, the observer will be protected from the weather. The same contrivance may also be used for the complete exclusion of all light. A foot-warmer filled with boiling

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXI. *April* 14, 1871. No. 6.

William Lassell, Esq., F.R.S., President, in the Chair.

Jas. Whitbread Lee Glaisher, Esq., Trinity College, Cambridge;
Capt. Wm. Campbell, attached to the Great Trigonometrical Survey, India; and
Chas. Coppock, Esq., 38 Arthur Road, Holloway,

were ballotted for, and duly elected Fellows of the Society.

Observations of the Zodiacal Light, 1850, *January to April*. By W. R. Birt, Esq.

The following *unpublished* observations of the zodiacal light were made in the earlier months of the year 1850 at the Kew Observatory, a locality especially favourable for observations of the kind. In them will be found the positions and progression of the "light" from month to month. I am not aware that so full and consecutive a series of observations have as yet been presented to astronomers; such may, however, exist, and if so, this series may be valuable for comparison with them.

The observations were commenced on January 7, 1850, when a faint, but very distinct, light was observed in the S.W. part of the heavens; it possessed a triangular form, the apex being near the planet *Saturn*, which then was not far from the first point of *Aries*; it was less bright, and more diffused, than the Milky Way, then very distinct.

January 13, 7.0. The light was seen under *Pegasus* extending rather beyond the planet *Saturn*. The dark space of sky between it and the Milky Way was very distinct.

January 30, 6.35 to 7.50. During this interval the "light" was very brilliant, but brightest towards the horizon. A line drawn through β and α *Pegasi* passed through the brightest

portion. A line from *Saturn* to the horizon, intersecting the former line, gave the principal direction of the "light" through which the planet was seen shining. The apex was situated about half way between the planet and the constellation *Aries*, and the "light" extended to about 0·66 of the distance between *Saturn* and γ *Pegasi* from the planet. Elongation about 67°.

February 3, 6.45 to 7.45. The lines drawn on the 30th of January indicated as then the general direction and brightness of the "light." The upper edge which previously had been noticed as parallel with α and γ *Pegasi*, on this evening was nearer to γ, and further from α, the greater portion of the "light" being between *Saturn* and γ *Pegasi*, with the planet within it. The lower edge was indicated by a line drawn from a point midway between *Saturn* and *Aries*, a little to the east of the planet directly to the horizon. Towards the close of the observation two small cumulus clouds passed between the "light" and the eye of the observer. The effect was immediately to give a diffused character to the "light," and to efface the triangular outline previously noticed.

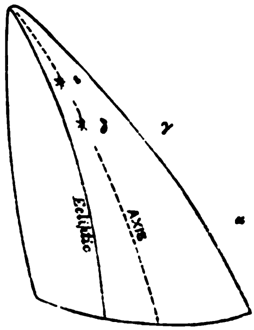
February 6, 7.20 to 8.0. "Light" very distinct, sky having cleared after a violent gale from W. and N.W. The advance of the apex was very perceptible, being distinctly traced to 21° of *Aries*. *Saturn* was situated within the "light," not far removed from the lower edge. Taking the distance between the planet and γ *Pegasi* equal to unity, the light extended to about 0·75 from the planet, and probably extended to about 0·25 of the same distance south of *Saturn*. The diminution of light from the axis towards the lower edge, and the dark space of sky on the eastern side was very apparent, as well as the direction of the upper edge from the apex situated on or near the ecliptic, between the stars η and ι *Piscium*, near γ *Pegasi*, towards the horizon, leaving α *Pegasi* to the west.

February 7, 7.15. Apex near π *Piscium*. Elongation, 66°.

February 12, 7.0 to 8.30. On this evening the "light" was more distinct and brilliant than it had yet been observed. The whole space between *Saturn* and γ *Pegasi*, except about 0·125, or rather less, was illuminated with a soft and delicate light. The planet was removed from the southern edge, but within the light, about the same distance as γ *Pegasi* was removed from the northern without it. The apex was situated about midway between the stars η and α *Piscium*, near ο *Piscium*, and the axis descended from this point between γ *Pegasi* and *Saturn*, about 0·66 from the planet towards the star. The northern edge passed from the apex near γ and α *Pegasi* to the horizon. The appearance of the light, as contrasted with the Milky Way, was very striking, especially in its rich, soft, and glowing character. It was remarked that on every occasion that the light had been observed, it was found to be strongest just as the evening twilight faded out. Elongation, 62°.

February 13, 7.5. "Light" very distinct and beautiful.

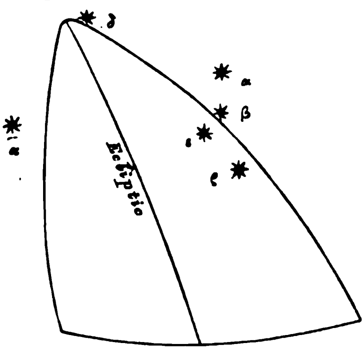
It stretched upwards from the horizon at an angle greater than that at which the equinoctial meets it, and less than that which the ecliptic forms with it. The apex had arrived at a line joining the stars β *Arietis* and α *Piscium*, equal to an elongation of 64°. The following is the note made at the time:—"The axis is certainly not coincident with the ecliptic, the space between *Saturn* and γ *Pegasi* being nearly filled with light, so that the great body of the light is north of the ecliptic. The more probable direction of the axis would be from 29° of *Aries* past ι and δ *Piscium*, crossing the equinoctial colure at 5° of north declination, and meeting the equinoctial at about 340° of right ascension.



Horizon. 1850, Feb. 13

March 3, 7.30. The extent of the "light" on this evening was found by drawing a line from α *Arietis* to α *Ceti*. It extended 0·60 from α *Arietis* towards α *Ceti*. The northern edge passed very near to γ *Arietis*. It was remarked that the greater portion of the light was, as in the middle of February, north of the ecliptic, apex near 15° of *Taurus*. Elongation, 62°.

March 4, 7.30 to 8.25. The light was very distinct, but not so brilliant as on the 13th of February. The northern edge was seen to rise from W.N.W., between γ *Pegasi* and α *Andromedæ*, nearest the former star, about 0·25 of the distance between them from it. It passed through the tail of the northern fish, not far from ϵ *Piscium*, directly over β and γ *Arietis* to δ *Arietis*. A line drawn from γ *Arietis* to α *Ceti* gave the extent of the light, which fills about 0·75 of the distance between the stars. Apex 18° of *Taurus*. Elongation, 64°.



Horizon. 1850, March 4.

March 6, 7.15 to 8.38. The northern edge passed close to, if not over β *Arietis*, and the "light" extended therefrom towards α *Ceti*, apparently as far as 0·80 of the distance between the stars; it also extended 0·90 of the distance from β *Arietis* to ξ and o *Tauri*. The greatest intensity of the light was observed to be about midway between the line from β *Arietis* to the equinoctial colure, and exceeded that of the Milky Way. α and γ *Ceti* and α *Arietis* were beyond the confines of the "light." The

apex was situated about midway between δ *Arietis* and the *Pleiades*. It was situated in 23° *Taurus*. Elongation, 67°.

March 11, 7.30 to 8.28. On this evening the "light" was described as very distinct and beautiful, very soft and glowing in its character, but not so brilliant as on the 13th of February. In shape it appeared to assume somewhat of a more rounded form than it presented during the earlier observations, especially on its southern edge, and its axis coincided more closely with the ecliptic than it did at those times. The stars in its neighbourhood, but beyond its outlines, were the following :—The *Pleiades* just beyond the apex ; on the north the constellation *Musca* (some distance from it), and α *Arietis;* on the south ξ and ο *Tauri*, α *Ceti*, and γ *Ceti*. The extent of the light was as follows : β *Arietis* to α *Ceti* 0·8. β *Arietis* to ξ and ο *Tauri* 0·9. β *Arietis* to γ *Ceti*, nearly, if not the whole space. The brightest portion, which more or less coincided with the axis, passed from a little south of the *Pleiades* towards the horizon crossing the line from β *Arietis* to α *Ceti* at about 0·4. The northern edge passed very obliquely between α and γ *Arietis*, near to and just to the north of β *Arietis*.

March 12, 7.45 to 7.55. Although the stars were dim, as if covered by a very thin sheet of cirro-stratus, and the light was not bright, about half the lustre which it exhibited on the 13th of February, the outline was distinct. The apex extended nearly as far as the *Pleiades*, and the northern edge passed very obliquely between α and β *Arietis ;* the southern edge appeared to have approached nearer to ξ and ο *Tauri* and to α *Ceti*.

March 13, 7.30 to 7.50. Although the light was very distinct, it appeared of a pale, whitish colour, and during the 20 minutes it was under observation not the slightest change in its intensity was detected ; it continued of the same lustre the whole of the time, shining with a steady radiance. The smaller stars were scarcely visible, and β and γ were perceptibly dimmer than when seen exterior to the light. The *Pleiades* appeared as the visible apex ; but a careful scrutiny resulted in the detection of their being slightly north of it.

March 13, 8.0 to 8.8. During these eight minutes well-marked intermissions in the luminosity of the light were detected. They were not of the nature of pulsations in the usual acceptation of the term, but consisted of alternate brightenings and dimmings of the entire mass, such as might be produced by the approach and recess of a luminous body. These alternations made themselves evident by the disappearance of their luminous edges, α and β *Arietis* for an instant or so appearing perfectly free from light, and then the edge extending again nearly as for as α. Nothing of this kind was seen between 7.30 and 7.50. It was particularly remarked that the great mass of light occupied about 0·5 of the line between β *Arietis* and α *Ceti*, or north of the Ecliptic, and this has been characteristic of the observations throughout.

March 15, 8.0 to 9.15. The "light" was very distinct, but rather pale and shining with a steady radiance, the brightest portion being west of the line joining β *Arietis* and α *Ceti*. The northern edge was nearer to α *Arietis*, and the southern had approached α *Ceti* and ξ and ο *Tauri*, distance from β *Arietis* to α *Ceti* 0·85, and to ξ and ο *Tauri* 0·95, apex 26° of *Tauri*, elongation 61°.

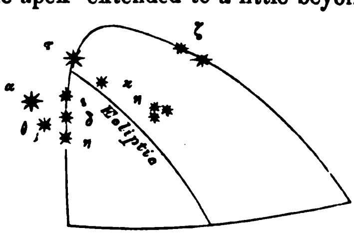
March 29, 8.15. Apex near κ *Tauri*. Northern edge passed a little north of the *Pleiades* through the constellation *Musca* and north of α *Arietis*. The southern edge passed about 0·66 of the line from the *Pleiades* to *Aldebaran*, nearest the latter star. On the same line the northern edge was about 0·125 of its extent *beyond* the *Pleiades*.

March 31, 8.30 to 8.50. A very strong Auroral light was seen in the magnetic north, which appeared to exert a decided and unmistakable influence on the Zodiacal light, the apex being still near κ *Tauri*. The light of the Zodiacal light appeared to be similar to that of the Aurora, although it was not so bright as that, it was quite as bright, if not brighter, than on the 12th and 13th of February. The distinction between the Aurora and the Zodiacal light was well marked.

April 2, 8.20 to 9.0. The "light" was very beautiful; its richness and softness being considerably greater than observed on the 12th and 13th of February, and its glowing character gave it an appearance which, for a mild radiance and chastened brilliancy, is seldom surpassed. The apex being still situated near κ *Tauri*, the northern edge passed about 0·5 the distance between ζ *Persei* and the *Pleiades* (which were quite involved in the light) through *Musca*, north of α *Arietis* to the horizon. The southern edge passed from the apex about 0·66, from the *Pleiades* to *Aldebaran*, thence to near ξ and ο *Tauri*. During the last 10 minutes the light appeared occasionally to extend as far as ζ *Persei*.

April 5, 8.45 to 8.50. Light not so brilliant. Apex extended as far as τ *Tauri*. Northern edge passed about 0·5 between the *Pleiades* and ζ *Persei*, the southern edge 0·66 from the *Pleiades* to *Aldebaran*.

April 10, 8.30 to 9.15. The apex extended to a little beyond τ *Tauri*, a line drawn from the star through the *Pleiades* was more or less coincident with the brightest portion of the light, which extended as far as ζ *Persei*. The northern edge was well marked, and could readily be traced from the apex past ζ *Persei*, and the constellation *Triangulum* to the horizon. *Aldebaran* was seen beyond the confines of the light to the south.



HORIZON. 1850, April 10.

Thus far the observations of 1850.

On the evening of April 7, 1871, at 9.15, I obtained a very favourable view of the Zodiacal light, and described it as a very soft glowing light, quite as bright, if not brighter, than the Milky Way. The *Pleiades* were involved in it, and appeared to be in its axis, or nearly so. The northern boundary passed near the star ζ *Persei*, and the southern boundary, close upon γ, δ, and ι *Tauri*. The apex, which appeared blunted and ill defined, extended to κ *Tauri*, and was directed towards the planet *Jupiter*. *Aldebaran* was quite clear of the light. On the evening of the 10th of April, 1871, the form and character of the "light" were identically similar to the appearance which it presented on the 10th of April, 1850.

There are two very prominent features that present themselves in connexion with these observations. The position of the great mass of light being constantly north of the Ecliptic, and the apparent change in the *form* of the "light," or at least that portion of it which forms the apex of the luminous triangle or cone. This is very perceptible in the groups of observations, those in February presenting a narrower cone of light, the axis being very decidedly *inclined* to the Ecliptic, although the apex was seen more or less on or near the Ecliptic. This group of observations is in decided contrast with that of a month later—viz., March. By this time the cone of light had become larger, the apex more rounded, and the inclination of the axis to the ecliptic changed; the greatest extent of light being still northward of the Ecliptic. In April the progression of the widening of the cone was very apparent; if equal distances from the apex along the axis be taken for February, March, and April, and the breadth of the cone measured; as indicated by the position of the "light" among the neighbouring stars, the differences of the three sets are so distinct as to lead to the suspicion that we view the phenomenon differently as the Earth advances in her orbit from the point at which we beheld it in the winter months.

On a Remarkable Appearance during the Solar Eclipse, Dec. 22, 1870. By R. W. H. Hardy, Commander R.N.

I have seen no account in any of the published reports of the late Solar eclipse of a remarkable appearance which attended it, and which was witnessed by the Rev. Henry H. Winwood, M.A., of Bath, and by myself from my Garden Observatory.

Some time after the commencement of the eclipse the sky here became partially overcast with two strata of cirrous clouds arranged at different elevations, one above the other. The lower stratum was much broken, leaving larger spaces bare. But neither of these strata offered any serious obstruction, for a considerable time, to our observation of the eclipse.

A few minutes—three or four, as nearly as I can remember—before the central epoch, the attention of Mr. Winwood and myself was drawn to a coloured patch which appeared, at the distance eastward from the Sun of about 15°, and which seemed to be stationary. The space occupied by it might be from its margin nearest the Sun, about 30° towards the East, and spreading to double that angular space N. and S.

The marginal portions next the Sun presented a lively ruddy tint, then melting into an orange tint, blended with a green and dark purple, in succession, proceeding eastward, and to the N. and S. Green however was the prevailing tint, and covered a much larger space than the other. Indeed, it seemed to be everywhere in the patch mixed with them, and to give them a muddy hue. These colours attained their greatest vividness about the time of the central phase of the eclipse. It soon after that began to fade in brightness, but to become more dark, especially towards the South-west. At $12^h\ 10^m$ or $12^h\ 15^m$ G.M.T. the clouds thickened, and we saw no more of the coloured patch, or even of the Sun, till about 2 P.M., when nothing remained of the former but a degree of obscurity towards E. and S.E.

I can hardly believe that this remarkable feature of the Solar eclipse, connected, as it would seem to be, with the Moon's shadow, was visible to only two private observers in our locality.

Kilkenny House, Sion Hill, Bath, 27th March, 1871.

P.S. Had the shadow of the Moon fallen upon the Earth instead of on the cloud, we should not have been able to perceive it, by reason of the distance from us at which it would have fallen.

A further communication has since been received as follows:—

In reference to the account I lately sent of the appearance of a coloured phenomenon seen by Mr. Winwood and myself in Bath, about the time of the central phase of the solar eclipse of December last, and which colours covered a considerable space on an upper stratum of cirrus clouds extending to the south-east of the Sun's apparent place, I omitted to call attention to a similar coloured phenomenon witnessed by Mr. Glaisher on his balloon ascent from Wolverhampton on August 18, 1862, and which he thus describes: "On looking over the side of the car, the shadow of the balloon *on the clouds* was observed to be surrounded by a kind of corona tinted by prismatic colours." Vide Glaisher's *Travels in the Air*, p. 49, published 1870. The italics are mine.

Now, if for *balloon* we substitute the word *Moon*, and *vice versâ*, the two cases will be found to be strictly analogous. For if both phenomena could have been witnessed from the Earth at the same time, it cannot be doubted, I think, that the identity of *cause and effect* would at once have been recognised. I further beg to state that all shadows of opaque bodies in Sun-light are

characterised by similar coloured phenomena, and which, so far as I know, all writers on optics and on harmony of colour have hitherto failed to notice. And yet it requires only a superficial observation to perceive them at and near the borders of the shadows of houses, even in the streets of London, on a sunny day.

Kilkenny House, Sion Hill, Bath, 21st April, 1871.

On the Zodiacal Light. By Josiah Rees, F.R.A.S.

(*Extract of a Letter from the Rev. W. A. Jevons.*)

The following is an extract from a letter received by me from the Rev. W. A. Jevons, of Liverpool, giving an account of an appearance in the heavens observed by him on the evening of Good Friday last at Buxton:—

Extract.—" We think we saw the Zodiacal light on Good Friday evening. When we first observed it, it was as nearly as I can tell (for my watch was slow) about 7^h 4^m, and it lasted about five minutes. It was a little to the N. of where the Sun had set behind a hill, and the ray of light was nearly perpendicular, and as nearly as I could guess without means of measurement, about a degree broad and five degrees long."

6 Fig Tree Court, Temple, E.C., April 11th, 1871.

Theoretical Considerations respecting the Corona. By Richard A. Proctor, B.A., Cambridge. In Two Parts. Part 1.

It may be questioned whether we are yet in a position to theorise safely respecting the Corona; yet I feel that I need offer no apology for entering here upon the analysis of the evidence now available on the subject, with the object of determining towards what hypotheses that evidence appears to tend. Having lately had occasion, while preparing my treatise upon the *Sun*, to go through the principal records of former eclipses, I shall be able to avoid the mistake of giving *undue* weight to observations lately made;—that is, of giving them a value founded rather on their recentness than on their specific importance. It has always seemed to me specially necessary, if one could theorise safely, to attach proper weight to *all* the known facts; and I have sometimes been led to believe that the want of success with which, as a rule (save in a few highly exceptional cases) observers theorise on their special subjects, is to be looked for in the fact that their own observations acquire an exaggerated importance in their minds, the labours of others being unduly, though quite honestly under-

rated. Precisely as the workers in some great edifice are not well placed to recognise its proportions as a whole, so it must commonly happen that the most skilful workers in science are precisely those who are least fitted to judge of the position to which—by others' labours and their own—their special subject has attained.

Certainly, there has been much to suggest such considerations in some recent enunciations of opinions respecting the corona. Many of the able spectroscopic workers who have dealt with the subject seem to regard spectroscopy as almost the only means of attacking the problem; experts in polariscopic analysis have at least not undervalued their special department of research; Mr. Brothers is convinced of "the pre-eminent use of photography for determining points in dispute;" and some general observers appear to consider the two minutes' view they have had of coronal phenomena more likely to supply a correct answer to all our questions than either spectroscopy, polariscopy, or photography,—to say nothing of the general observations made by others, or of the voluminous records of former eclipses.

If we consider what *new* information the recent eclipse has brought us, and combine that information with what had previously been ascertained, we shall probably have a better chance of arriving at satisfactory results, than by limiting our attention to a few disjointed facts. Fortunately the attention of those, even, who are least familiar with the history of coronal research, need now no longer be distracted by any theories tending to explain the corona as a phenomenon of our own atmosphere. Men of science are now in (practically) unanimous agreement as to the solar nature of the corona; so that we can give our whole attention to the much more difficult, and much nobler problem, "What is the real nature of this great solar appendage which total eclipses reveal to our view?"

I have long entertained the belief that the solar corona is due, in great part if not wholly, to the existence of millions of meteoric systems having their perihelia for the most part much closer to the Sun than our Earth's orbit. The evidence recently obtained induces me to think that I have somewhat exaggerated the part which must be assigned to meteoric systems in accounting for coronal phenomena. Of course there can be no question about the existence of enormous numbers of meteoric systems in the position specified. It would be utterly unreasonable to suppose that the fifty-six meteoric systems recognised by Prof. Alexander Herschel, and those others (making up the number to more than a hundred) which Heis has recognised, bear any but a most minute proportion to the total number having perihelia within the Earth's orbit. The laws of probability will not permit such an assumption for a moment. Nor can it well be doubted that the density of perihelion-distribution increases towards the Sun's neighbourhood, in the case of meteors, precisely as in the case of comets; insomuch that portions of many meteoric systems would be much more intensely illuminated (surface for surface) than our Earth, or

Venus, or *Mercury*; while in many cases myriads of meteors near the perihelia of their orbits must be rendered incandescent, if not absolutely vaporized, by the intensity of heat to which they are exposed. Even setting this probability out of the question, it remains—not a theoretical—but a demonstrated fact, that no inconsiderable quantity of light must come to us during total eclipse, from meteors lying towards the Sun's place and illuminated by his light.

But although we have thus undoubtedly found a *vera causa* for a portion of the coronal light, yet there are phenomena which seem to prove that another and larger portion remains unaccounted for. In the March number of the *Notices*, I have pointed out the most marked peculiarity of the sort, the observed association, namely, between the expansions and depressions of the inner part of the corona, and those far reaching radiations which form the principal feature of the outer corona. This is most strikingly shown in the negative of Mr. Brothers' best photograph, and is confirmed by Lieutenant Brown's drawing. But it is worthy of notice that during the eclipse of 1869, evidence was obtained which tended almost as strongly to establish the same association; and I am disappointed with myself, that though I long and carefully examined the records (photographic and otherwise) of that eclipse, this important fact escaped my scrutiny. Now there is another relation, scarcely less significant, when considered alone, but assuming a yet greater importance when combined with the last. Wherever the inner portion of the corona is depressed, there the coloured prominences are wanting and the sierra itself is shallow. Professor Roscoe, speaking of Mr. Seabroke's maps of the prominences (made before the eclipse), and Professor Watson's drawing of the inner corona, says:—"On comparing the two drawings thus independently made, a most interesting series of coincidences presented themselves. Wherever on the solar disc a large group of prominences was seen in Mr. Seabroke's map, there a corresponding bulging out of the corona was chronicled on Professor Watson's drawing; and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the Sun's limb. It of course follows, by combining the two relations, that the prominences are most numerous where the corona extends farthest, a fact noticed by Mr. Brothers, who tells us that throughout totality (as evidenced by his photographs), there was more coronal light on the west side of the Sun than elsewhere, and further, that "the prominences were more numerous on the side where the corona was brightest."* Indeed, on this side there was

* The photograph of Lord Lindsay's series, which was exhibited at the January meeting seemed to show the reverse. It appears, however, on a careful comparison of that photograph with Mr. Brothers', and the American one, that in some as yet unexplained way Lord Lindsay's has been inverted and reversed. The perfect agreement of the prominences when this correction is attended to leaves no doubt that such a mistake has been made.

seen towards the close of the totality, a perfect sickle of prominences,—according to the account given by Father Secchi.

Now the point to be specially attended to here, is the evidence of vertical disturbance (vertical with reference to the Sun's globe) extending to enormous distances from the photosphere. The notion that we have to do with objects of the nature of concentric atmospheric shells—advanced (as it seems to me) in the first place on very insufficient evidence,—appears completely negatived. If the inner corona had presented no signs of association with the outer corona, we might have overlooked the very marked departure of the former's outline from concentricity with the Sun's, precisely as for several months the irregularity of the *sierra's* outline was overlooked or forgotten, and the atmospheric-shell character ascribed to it (by implication) in the adoption of the title *chrom(at)osphere*. But the observed association between the inner corona, and so obviously unshell-like an appendage as the outer radiated corona, leaves the shell theory implied by the title *leucosphere*, altogether untenable. We might *fairly* claim from an atmospheric shell a respectable smoothness of outline, but we can authoritatively claim that it shall not associate itself, even in appearance, with a strikingly radiated and gapped appendage.

But equally the meteoric theory of the corona must be abandoned,—at least so far as its claims to account for the special features of the corona (and therefore, for the greater part of the coronal light) are concerned.

We seem to have unmistakable evidence of the action of vertical solar forces, or at any rate of forces directed outwards from the Sun's globe,—though not necessarily exactly radial.

At the outset, the air of improbability which unquestionably surrounds this theory,* is to a certain extent removed by what we have learned respecting the formation of prominences. The evidence supplied by Zöllner and Respighi, to whose labours in this special department astronomy owes so much, suffices to show that prominences, as respects their first formation, are phenomena of eruption. For although Zöllner has divided the prominences into two classes,—the cloud-prominences and the eruption-prominences, —yet he in no sense negatives the statement of Respighi, that the appearance of prominences is preceded by the formation of a rectilinear jet, either vertical or oblique, and very bright and well defined. Respighi adds, that a jet of this sort, "rising to a great height, is seen to bend back again, falling towards the Sun like the jets of our fountains, and presently the sinking matter is seen to assume the shape of gigantic trees more or less rich in branches and foliage."

The velocity with which the gaseous matter of these prominences must pass the photosphere, in order to reach so great a height above it as Respighi has noticed in the case of some prominences, is as nearly possible 200 miles per second, even if we neglect all the effects of resistance as the erupted gas rushes on-

* It will be known to most of those who read this paper that the theory is by no means a new one.

ward to the highest point of its excursion. Let this be specially noted. If the highest prominence yet seen by Respighi was the highest possible, and was wholly unforeshortened, we yet have proof of an eruptive action capable of sending out gaseous matter with the enormous velocity mentioned above; and this, be it remembered, is only the velocity with which the erupted matter crosses the level of the photosphere. Far beneath that level, at those depths, for instance, whence Zöllner assumes the prominence-matter to have been erupted, the velocity must have been far greater. If we also take into account the effect of the resistance which is undoubtedly encountered by the erupted gas during its flight, we have to add even more largely to the initial velocity. It is scarcely *conceivable*, all such considerations being duly weighed, that the initial velocity can be less than 300 miles per second; and it is far from inconceivable that the initial velocity may be two or three times as great.

So far, be it remembered, we have been dealing with known facts, and the only way of avoiding the general conclusion above indicated, is by rejecting the supposition that prominences are phenomena of eruption. This will scarcely be regarded, however, as permissible, in the face of the reasoning of Zöllner and Respighi, and the evidence which they have adduced in its support. And besides we have independent means of knowing that *outside* the photosphere velocities occur which are comparable with those above referred to. I may cite for instance the motion of the bright points watched by Carrington and Hodgson in 1859, as well as the spectroscopic measurements of the velocity with which portions of the solar atmosphere are at times endowed.

Now, if velocities such as I have spoken of are produced by eruptive action exerted far beneath the photosphere, then there can be no question that any material erupted with the glowing hydrogen, and of considerably greater density, would retain, when passing the level of the photosphere, a much greater proportion of the velocity initially imparted. For example, instead of the velocity of 200 miles per second with which the matter of Respighi's great prominence *must* have crossed the photospheric level (to attain its observed height, any solid, liquid, or even dense gaseous matter flung out with the glowing hydrogen of that prominence, would probably retain a velocity of—say—240 miles per hour. In such a case it would reach a height exceeding that indicated by the greatest extension of the radiations observed last December. A velocity (at the photospheric level) only one half greater than this would, indeed, suffice to carry a body to a distance from the Sun equalling our Earth. A velocity twice as great as that of the prominence-matter at the photospheric level, would carry a body for ever away from the Sun, never to have its velocity reduced to less than 125 miles* per second, even when it had passed away to stellar distances.

* The velocity here spoken of is one of 400 miles per second; the least velocity required to carry a body altogether away from the Sun, is, at the *photosph*[illegible]0 miles (more exactly 379) per second. The velocity of

But certainly the theory that a large proportion of the coronal light is due to matter erupted from the Sun, is one which would require very strong evidence to render it acceptable. We have seen that the appearance presented by the inner and outer corona and the observed association between those regions and the sierra, lead directly to the theory that the corona is a phenomenon of eruption; and undoubtedly this theory is the one naturally suggested by the photographic views of the corona in this and the last eclipse, as well as by the drawings and descriptions of the corona as seen in preceding eclipses. It is impossible for instance, to look either at the drawing made by Mr. Gilman of the eclipse of 1869, or at the very remarkable (and most strongly attested) picture of the corona as seen during the eclipse of 1868 at Mantawaloc-Kekee, without being impressed with the feeling that we have here pictures of eruptional phenomena. Still, all such natural and obvious conclusions are to be regarded by the true student of science with great distrust, and to be analysed with exceeding care, the natural senses being of all his faculties those which are most likely to lead the theoriser astray.

It will be well then to inquire whether the somewhat startling theory here dealt with is confirmed or disproved by such tests as we can at present apply to it.

The first and most obvious test is the examination of the photographic records, to see if any signs of the action of eruptive forces can be detected in the corona. Here I am disposed to lay great stress on the examination of the original negative of Mr. Brothers' best picture, No. 5, and it was with some interest that I availed myself of his kind permission to scrutinise it. The evidence it supplies is such as none of the positives supply, such as no artist perhaps could reproduce. It is such as to suggest in the strongest possible manner that the coronal radiations are phenomena of eruption. I was pleased to find, when I mentioned this view to Mr. Brothers, that Mr. Baxendell, of Manchester, had expressed precisely the same opinion of this remarkable record.* The appearances referred to are such as to show that some pictures of the corona as seen under exceptionally favourable conditions, are not, as has been commonly supposed, altogether idealized and in fact unwarranted, but merely represent with exaggerated distinctness features which have a real existence. This remark applies, for instance, to that remarkable drawing by

a body which crossed the photospheric level at the former rate, would at an infinite, or practically infinite distance, be reduced to $\sqrt{(400)^2 - (380)^2}$, or about 125 miles per second.

* I venture to express here my feeling that it is of the utmost importance that the evidence given by this photograph should be exhibited as fully as possible,— in the Report of the Eclipse,— even though it should appear that this could only be done at considerable cost. The extreme delicacy of details is such that nothing but the perfection of engraving can properly exhibit their nature. But I do not hesitate to say if they *can* be reproduced, they would be invaluable, in the present state of our knowledge respecting the corona.

Liais, of the eclipse of 1858, a copy of which appears at p. 326 of my treatise on the Sun, as also, though in a less degree (the exaggeration being in these instances less marked) to the picture by Feilitsch (p. 330), and that of the corona as seen at Mantawaloc-Kekee in 1868 (p. 334). As respects this last eclipse I would invite special attention to the account given by Mr. Pope Hennessey, who observed at a station near Labuan. The luminous ring round the Moon "was composed," he says, "of a multitude of rays quite irregular in length and in direction; from the upper and lower parts they extended in bands to a distance of more than twice the diameter of the Sun. Other bands appeared to fall towards one side; but there was no regularity, for bands near them fell away apparently towards the other side. When I called attention to this, Lieut. Ray said, 'Yes, I see them; they are like horses' tails;' and they certainly resembled masses of luminous hair in complete disorder." (The reader will be reminded here of the appearances resembling hanks of thread in disorder, seen during the eclipse of 1842.) The account is accompanied by a drawing precisely resembling such a drawing as I should make if I were to try to represent what I saw in Mr. Brothers' negative.

But now, leaving on one side the peculiar forms of the bands and rays, I would note that as to the existence of fine rays in the structure of the corona close by the Sun's limb, we have other evidence of a very striking nature. I refer to Mr. Gilman's picture in the report of the eclipse of 1869. In the letter from Sir John Herschel, read by Mr. Brothers at the March meeting, there occurs this passage, referring to Mr. Brothers' photograph:—"I see nothing which gives me the idea of rays or streaky radiation such as appear in Gilman's picture, which, if it *could* be believed, would point to lunar mountains as the origin of the dark spaces, and bring the whole phenomenon within the distance of the lunar orbit." Now we have seen that Mr. Brothers' negative *does* show precisely such rays, and furthermore Mr. Gilman's narrative is far too distinct, as is also that of Mr. Farrell, who observed with him, to leave any doubt as to the reality of the phenomenon. "The corona," says Mr. Gilman, "was composed of an infinitude of fine violet, mauve-coloured, white, and yellowish-white rays, issuing from behind the Moon." The corona "looked," says Mr. Farrell, "as if it was the product of countless fine jets of steam issuing from behind a dark globe." We cannot reject such testimony as this, coming from undoubtedly competent observers, and altogether inexplicable as due to mere illusion. Whatever conclusions this observation (as well as Mr. Pope Hennessey's, Lieut. Ray's, and the photographic negative) may seem to point to, must unquestionably be examined with attention. But the lunar explanation was not only negatived by the evidence obtained during the recent eclipse, but is completely disposed of by the considerations I adduced in the January number of the *Notices*. I have since had occasion to submit those considerations to Sir

John Herschel, leaving him to judge of their weight, and he replied at once that, as I urged, any illumination derived from cosmical dust on *this* side of the Moon would be altogether *lost* in the illumination derived from the cosmical dust lying beyond, up to, and past the Sun's place. But it will be seen, at once, that the theory which regards the corona as a phenomenon of eruption, requires that these "countless fine jets," &c., should be seen, whenever the corona is viewed under exceptionally favourable conditions. The rush of matter cannot be conceived as taking place otherwise than in countless exceedingly fine jets, in jets corresponding perhaps in number to the countless minute prominences which produce the *sierra*. And in passing it must be noted that the actual quantity of erupted matter must be regarded as inconceivably minute by comparison with the Sun's mass, insomuch that the whole amount of matter present at any one moment in the corona (so far as this cause is concerned) might be outweighed by one of the least among the asteroids.

But it may now be well to remove certain difficulties which present themselves when the startling theory we are dealing with is carefully considered,—or rather it remains to show that the considerations on which such difficulties are founded tend in reality to afford striking evidence in favour of the theory.

In the first place, it will seem highly probable that some at least of the matter flung out from the Sun would have such velocities as I have referred to above as surpassing those required either to reach the Earth, or even to carry matter away altogether from the Sun's control. In this case, it would follow :—1st, that some of the erupted matter would from time to time salute our Earth ; and, 2ndly, since other stars are suns like our own and may be presumed to behave in a similar manner—that matter erupted from the stars might cross the interstellar spaces and visit our own system.

Now regarding the erupted matter as, after cooling, meteoric in nature, it would undoubtedly follow from the first of these results, that more meteors should fall in the daytime than at night. For the night hemisphere of the Earth would be saluted only by meteors not belonging to these solar eruptions. In the daytime solar meteors (if one may so term them) would be added, since meteors arising from the Sun must needs fall on the hemisphere turned towards him at the moment of the arrival ; there would be a preponderance, then, in favour of day-falls. Now the only class of meteors we can make comparison by, is the class of aerolites, since the fall of these only can be recognised in the day time. And it is a fact, according to the testimony of Humboldt, Heis, and others, that these aerolites fall, on the whole, somewhat oftener in the daytime than at night.

But, if some meteors ejected in this way from *stars* should reach our system, we should expect that they would exhibit some signs of having been expelled with a velocity exceeding that barely necessary to carry them away from their parent Sun. In

other words, we should expect that *some* meteors would exhibit velocities exceeding those which the Sun can impart to masses drawn by him from outer space. Now it has been a source of grave perplexity to all who have studied the details of meteoric astronomy that some meteors *do* actually traverse our atmosphere with a velocity exceeding by many miles per second that which they could possibly have, even though after been drawn by the Sun from an infinite distance, they encountered the Earth full tilt at the moment when she is in perihelion. This velocity cannot cannot exceed 45 miles* per second, whereas we have satisfactory

* In such an inquiry the effect due to the Earth's rotation, as also that due to her own attraction upon the meteors, may safely be neglected by comparison with the much greater velocities of the meteor and of the Earth in perihelion, which we add to obtain the meteors' apparent or relative velocity. But it might seem as though, in some cases, the outer planets, and especially *Jupiter*, might impart an additional velocity to meteors passing near them on their way to the Earth, and that thus the observed excess might be accounted for. Now passing over the fact that only a few meteors have arrived on such paths as would at all correspond with the imagined explanation, the following considerations will suffice to show that the effect which even *Jupiter* would have in increasing the velocity of a meteor is not sufficient to account for the observed velocities. (It will be obvious that *Jupiter* and *Saturn* could never both act to accelerate a meteor in the imagined way, unless in cases so exceptional that not one instance could occur out of countless millions of observed instances):—

Conceive that while a meteor approaches the Sun from an infinite distance (*i.e.*, a distance practically infinite) *Jupiter* is at rest, in the course of the meteor, at a distance J from the Sun; and that at the moment the meteor is about to impinge on the surface of *Jupiter*, the planet is suddenly removed altogether away. Then in this imaginary case, it is obvious that *Jupiter* would produce a much greater effect than he can by any possibility produce in increasing the velocities of meteoric bodies. Now it is easy to determine what would be the velocity of a meteor, subject to the imagined influence, when it subsequently crossed the Earth's orbit. The equation of motion before the meteor reached *Jupiter's* place would be

$$\frac{d^2 x}{d t^2} = -\frac{\mu_0}{x^2} - \frac{\mu_J}{(x - \mathrm{J})^2},$$

where μ_0 is the attractive force of the Sun's mass at an unit of distance, μ_J that of *Jupiter's*. We get, then,

$$\left(\frac{d x}{d t}\right)^2 = \mathrm{C} + \frac{2 \mu_0}{x} + \frac{2 \mu_J}{(x - \mathrm{J})};$$

$\mathrm{C} = 0$, and putting j for the radius of *Jupiter*, the velocity (v) of the meteor just as it is about to reach the surface of *Jupiter* is given by the equation,

$$\left(\frac{d x}{d t}\right)^2 = v^2 = \frac{2 \mu_0}{\mathrm{J} + j} + \frac{2 \mu_J}{j}.$$

The equation of the subsequent motion is

$$\frac{d^2 x}{d t^2} = -\frac{\mu_0}{x^2},$$

giving

$$\left(\frac{d x}{d t}\right)^2 = v^2 = \mathrm{C}' + \frac{2 \mu_0}{x}. \qquad \text{(i)}$$

evidence of meteoric velocities of 70, and even up to 80 miles per second.

When we remember also the observed association between certain meteoric systems and comets, similar evidence will be recognised in the hyperbolic figure of some cometic orbits, since no comet approaching the Sun can possibly be caused to travel in a hyperbolic orbit by his attractive influence alone. The hyperbolic figure is proof positive that comets whose orbits exhibit that figure have entered the domain of our Sun, with considerable velocities imparted to them in some as yet unascertained way. As such a comet necessarily passes away from the domain of some other star with such velocity, it follows that neither has such

When

$$x = J + j,\ v^2 = \frac{2\mu_s}{J+j} + \frac{2\mu_J}{j},$$

so that

$$\frac{2\mu_s}{J+j} + \frac{2\mu}{j} = C' + \frac{2\mu}{J+j},$$

and therefore

$$C' = \frac{2\mu_J}{j};$$

hence (i) becomes

$$v^2 = \frac{2\mu_s}{x} + \frac{2\mu J}{j}. \qquad \text{(ii)}$$

Now in the case of a meteor crossing the Earth's orbit, after being brought by solar influence alone from infinity, the velocity would be given by the equation

$$v^2 = \frac{2\mu_s}{x},$$

when for x was substituted the radius of the Earth's orbit. We know that this velocity is about 25·7 miles per second. And again, it is easy to calculate the value of $\frac{2\mu J}{j}$, which is, in fact, the expression for the square of the velocity with which a body approaching *Jupiter* under his sole influence from infinity would reach his surface. This is easily shown to be rather less than forty miles per second. Hence (ii) becomes

$$v^2 = (25{\cdot}7)^2 + (40)^2$$

whence v is about $47\frac{1}{2}$ miles per second. This is a considerable increase, but when combined with the Earth's perihelion velocity of $18\frac{3}{4}$ miles per second, it amounts to but 66 miles per second, and therefore still falls considerably short of authenticated meteoric velocities. But it need hardly be said that the actual influence of *Jupiter* can never approach in value that above estimated on an imaginary hypothesis.

It is worthy of notice, and has an important bearing on meteoric astronomy, that the possible influence of *Jupiter* in increasing the velocity of bodies which have approached the Sun's surface from enormous distances is very much less; for equation (ii) becomes in this case

$$v^2 = (379)^2 + (40)^2$$

whence v is about 382, corresponding to an increase of but three miles per second.

[The following formula is convenient for comparing the maximum velocity with which a body moving from infinity would reach the surface of either of

other star by its attractive powers generated the whole of the comet's velocity. And as there is no limit to the application of such considerations, there seems no other way of explaining the interstellar velocities of the comets which have hyperbolic orbits, than by tracing back their course to the moment when their substance was ejected from some star with a velocity exceeding, by many miles per second, that with which a body would reach that star if attracted from an infinite distance by the star's sole influence. Granted that hyperbolic orbits exist, it is unquestionable that they are not *due* to the stellar attractions, however perfectly the motion of a comet in such an orbit corresponds, as we know, with the theory of gravitation.*

It would follow, if meteors or some meteors were star-expelled bodies, that their constitution, when examined microscopically or under chemical analysis, would exhibit some traces of their origin. In Part II. of this paper I propose to consider the very striking evidence we have on this point. I shall touch also on some other evidence in favour of the theory here dealt with—a theory which, startling as it appears, seems yet to accord, better than any other, with what is at present known respecting the corona.

On the Orbits of the Revolving Double Stars ζ Herculis and ζ Cancri. By W. E. Plummer, of Mr. Bishop's Observatory, Twickenham.

(*Communicated by G. Bishop, Esq.*)

ζ Herculis.

Though various attempts have been made to determine the elements of the orbit in which this short-period binary performs its revolution, none represented with precision the angle of position when the star had become observable after the close approach of the components in 1866. Mr. Breen's, the most recently published orbit with which I am acquainted, exhibited such large discrepancies between the observed and computed positions, that it was evident that his elements might be improved with the information afforded by more recent observations. By means of equations of condition founded upon the observations of

two globes under their sole influence respectively. Let the radius of one be R, and its mean specific gravity C_R; the radius of the other r and its mean specific gravity C_r; the respective maximum velocities being V and v. Then $\frac{V^2}{v^2} = \frac{R^2 C_R}{r^2 C_r}$. The same relation holds if V and v be the velocities acquired in falling from heights H and h respectively, where $\frac{H}{h} = \frac{R}{r}$.]

* We may suppose, indeed, that in some few instances (*i.e.*, relatively few), planets like *Jupiter* and *Saturn* may have given to parabolic cometic orbits a hyperbolic figure; but it seems scarcely admissible to suppose that this is otherwise than exceptional.

the best observers from 1782 to 1869, I have determined the elements which follow.

The observations of this long period of ninety years are fairly represented, but I have thought it useless to exhibit a long list of compared observations, and have computed a short Ephemeris for the next five years, with which comparisons may be instituted and observers decide upon the merit of the orbit.

Date of the Periastron	1866·241
Period of the Revolution	36·606 years
Longitude of the Node	27° 0'·4
Longitude of the Periastron	291 49·0
Inclination	50 14·1
Excentricity $= e = 0·55110, \phi =$	33 26·5
Mean Motion in minutes	[2·77090]
Mean Distance	1''·374

ζ Cancri.

I have also made a new determination of the orbit of *ζ Cancri* in the same manner as that of *ζ Herculis*, employing the elements given by Dr. Winnecke. The observations of the pair extend over a period of nearly ninety years, and all the observations that have been compared with the positions computed from the elements agree very closely. The observed distances, however, present considerable discrepancies, to be accounted for perhaps by the general closeness of the stars. Observers of acknowledged repute differ considerably in their estimation of distance; those measures have been employed which were taken when the stars were considerably separated as being probably the more exact.

Periastron Passage	1872·44
Period of Revolution	58·23 years
Longitude of the Node	15° 37'·4
Longitude of the Periastron	171 46·8
Inclination	36 14·4
Excentricity $e = 0·30230, \phi =$	17 35·8
Mean Motion in minutes	[2·56930]
Mean Distance	0''·908

The computed values are as follows :—

	ζ Herculis.			ζ Cancri.	
Date.	Angle of Position.	Distance.	Date.	Angle of Position.	Distance.
	° '	"		° '	"
1870·0	196 50	1·04	1870·0	196 44	0·65
1870·5	192 31	1·09	1870·5	192 6	0·64
1871·0	188 30	1·13	1871·0	187 22	0·64
1871·5	184 45	1·17	1871·5	182 30	0·63

ζ Herculis.			ζ Cancri.		
Date.	Angle of Position.	Distance.	Date.	Angle of Position.	Distance.
	° ′	″		° ′	″
1872·0	181 10	1·20	1872·0	177 30	0·62
1872·5	177 46	1·22	1872·5	172 23	0·61
1873·0	174 31	1·24	1873·0	167 8	0·60
1873·5	171 21	1·26	1873·5	161 43	0·59
1874·0	168 12	1·27	1874·0	156 5	0·58
1874·5	165 9	1·28	1874·5	150 13	0·57
1875·0	162 12	1·29	1875·0	144 7	0·56

On the Shallowness of the Real Solar Atmosphere. By Richard A. Proctor, B.A. (Cambridge).

In my treatise on the Sun I have pointed out at page 192 that the conspicuous nature of the darkening of the disk near the edge is a proof of the shallowness of the superincumbent atmosphere, and not, as is commonly stated, of that atmosphere's being enormously deep. And at p. 295 I indicate reasons for believing that the method by which the prominences and sierra have been studied when the Sun is not eclipsed, is not capable (save under highly exceptional conditions) of exhibiting the existence of the true solar atmosphere,—that atmosphere, to wit, which causes the dark lines in the solar spectrum.

It will be known to all who read this communication that Professor Young of America, and Mr. Pye, independently recognised the existence of a highly complex atmosphere close by the solar photosphere. The slit of a spectroscope being placed tangentially to the limb, at the place where second contact was to occur, the spectroscopic field at the moment of totality and for several seconds after, was seen to be full of bright lines, "every non-atmospheric line of the solar spectrum showing bright."

The accuracy of this observation has been called in question. It is urged that the method of observing the uneclipsed Sun should be competent to show these bright lines if the supposed atmosphere have a real existence.

Now, the competence of the last-mentioned method, so far as its power of obviating the effects of atmospheric illumination is concerned, cannot be questioned. For, indeed, as we know, Mr. Lockyer has, on one occasion, seen multitudes of the Fraunhofer lines reversed in this way. But because he has on all other occasions failed, while neither Zöllner, Respighi, nor Young has been favoured even with a single view of this sort, it is urged that no such atmosphere can exist, or that, at any rate, eclipse observers could have no better opportunity of recognising it with the spectroscope than those who have studied the uneclipsed Sun: I

would submit that this inference is erroneous, and that in one important respect observations made during eclipses have a great advantage over observations made when the Sun is not eclipsed. It has been overlooked, as I opine, by those who urge the objection I am considering, that the image of the solar limb, whether as viewed in the telescope, or spectroscopically, is formed by the combination of diffraction-images of the several points of the real limb; and, therefore (independently of irradiation, which, however, should also be taken into account), must needs extend beyond the true outline. We can tell, in fact, how great the extension is, since we know (experimentally) the dimensions of the diffraction-images of luminous points, for given apertures. A telescope which would not separate γ^2 *Andromedæ*, for instance, would *certainly* not be capable of showing (when armed with a suitable spectroscope) the bright lines of a solar atmosphere whose height subtend but about the fifth of a second—that is, of an atmosphere 80 or 90 miles in height; nor probably would it show these bright lines, even though the atmosphere were three or four times as high.

I think, therefore, that we are not justified in rejecting, or even in regarding as inconclusive, the observation made by Professor Young and Mr. Pye. The inference would clearly be that the sierra cannot in any sense be regarded as the true solar atmosphere. Be it noted, also, that the evidence here considered is altogether independent of that on the strength of which I have been led to assert that in all probability the sierra is not of the nature of a solar envelope at all, but is made up of multitudes of relatively small prominences and of the remains of larger ones. So far as I know, the only circumstance on which the theory that the sierra is an atmospheric envelope was founded, was the supposed smoothness of its outline. This relates to the observations which led to the re-discovery of the sierra in 1868. But telescopic observations by Airy, Leverrier, Secchi, and many others in 1847, 1851, 1860, &c., had abundantly established the fact that its outline is irregular. Respighi, in 1868, confirmed this with the spectroscope, and the fact is now generally admitted.

We have, I conceive, no escape from the conclusion that the prominences and sierra consist of glowing vapour which has been flung *through* the real solar atmosphere,—that atmosphere being highly complex and probably existing, especially near the photosphere, at an enormous pressure. What that medium is in which the prominences and sierra are seen remains to be shown. The idea may be suggested that it is of the same nature as the medium which we *call* the ether of space.

Observations of Amalthæa (113), *made at Bilk, near Dusseldorf.* By Dr. R. Luther.

(Extract of a Letter from Dr. Luther to the Astronomer Royal).

I have the honour to send you some observations of my new planet (113), of the 10–11 magnitude, for which the Berlin astronomers have proposed the name *Amalthæa*.

1871.	M.T. at Bilk. h m s	R.A. h m s	N.P.D. ° ′ ″	No. of Comps.
March 12	10 59 24·9	12 1 11·34	82 14 24·6	8
13	9 21 4·5	12 0 24·48	82 6 47·5	6
15	9 8 43·3	11 58 41·65	81 50 53·6	8
19	9 5 33·6	11 55 12·12	81 19 49·7	10
20	9 6 35·4	11 54 19·64	81 12 19·0	10
22	9 20 3·7	11 52 33·90	80 57 35·1	11
23	8 58 11·1	11 51 42·48	80 50 38·2	10
24	9 55 40·0	11 50 48·63	80 43 29·1	10

Elements of Amalthæa.

The following elements have been computed by Dr. Tietjen, from observations made at Berlin on March 12, 18, and 25:—

1871, March $25^{d}\cdot5$.	Berlin M.T. ° ′ ″
M	342 55 31·7
$\varpi - \Omega$	75 25 52·6
ϖ	198 59 27·2
Ω	123 33 34·6
ι	5 6 49·3
ϕ	4 38 28·3
μ	964″·224
Log a	0·377219
Sidereal revolution	1344·1 days.

New Comet.

(Extract of a Letter from Dr. Winnecke to Mr. Hind, dated Carlsruhe, April 8.)

I discovered yesterday evening, at about $8\frac{1}{2}^{h}$, a small telescopic comet in *Perseus*, the position of which with reference to a small

anonymous star of Argelander's *Durchmusterung* in its neighbourhood was estimated,

April 7	$8^h\ 40^m$ M.T.	R.A. $2^h\ 27^m\ 0^s$	Decl. $+53°\ 55'$
	Daily motion	$+\ 5\ \ 0$	$-\ 0\ 30$

Observation of Winnecke's Comet at Mr. Bishop's Observatory, Twickenham.

	Twickenham M.T.	R.A.	Decl.
	h m s	h m s	° ′ ″
April 10	8 35 9	2 41 40·07	+52 26 49·9

There was an evident extension of the nebulosity on the side opposite to the Sun, as if a tail might be expected on the comet's nearer approach. At present it will not be observed without a good telescope.

Elements of Winnecke's Comet. By Mr. Hind.

From the first observation of the new comet on the evening of discovery, and two by myself with Mr. Bishop's Equatoreal on the 10th and 12th, I have deduced the following elements of the orbit:—

T	1871, June 9·28137 G.M.T.	
π	$146°\ 19'\ 8''$	Apparent Equinox, April 10
☊	$280\ 53\ 32$	
ι	$86\ 50\ 54$	
log q	0·7854883	
	Motion direct.	

The comet appears to be quite distinct from any previously computed.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXI. *May* 12, 1871. No. 7.

WILLIAM LASSELL, Esq., F.R.S., President, in the Chair.

Henry Mann, Esq., Cleckheaton, near Normanton;
Clarence Edward Trotter, Esq., 33 Kensington Square;
Reginald Bushell, Esq., Hinderton Weston, Cheshire;
J. H. Blumm, Esq., H. M. S. Caledonia, Malta,

were balloted for, and duly elected Fellows of the Society.

On the Change in Colour of the Equatorial Belt of Jupiter. By John Browning, Esq.

The last number of the *Astronomical Register* contains a report of a discussion on this subject, at the monthly meeting of the Royal Astronomical Society. With the exception of Messrs. Ranyard and Penrose, the whole of the speakers considered that no change had taken place in the colour of the planet.

It was suggested that the reason the colour on the equatorial belt is now seen by many observers, while it was not seen in previous years, is to be accounted for by the fact that within the last few years many observers have become possessed of silvered glass reflectors of large aperture.

With all deference to such admirable observers as the speakers, I would beg to point out that such an explanation will not hold good in my own case.

Five years ago I began making careful coloured drawings of *Jupiter* with a reflector of 10½ inches aperture.

As long since as December 13, 1867, I drew attention to the fact that colour is best seen with small apertures or high powers.

I worked with powers from 350 to 500 whenever the air would permit. Although at that time I saw easily the coppery grey of the dark belts, and the bluish grey of the poles, I could detect no strong colour on the equatorial belt. Yet for the last two years the tawny colour of the equatorial belt has been more conspicuous than either.

It is true that during the last three years I have had a 12¼-inch equatorial reflector, but, owing to unfavourable atmospheric conditions, practically I have seldom indeed used more than 10 inches of aperture.

Several observers have seen the tawny colour of the belt with both refractors and reflectors of only three or four inches aperture.

The exact colour of the equatorial belt may be obtained by allowing a very powerful light to pass through a jet of steam, so that an increase in the luminosity of the body of the planet would completely account for the colour of the belt.

If, as I suspect, the colour appears periodically, we shall have probably to wait several years before we can decide this matter.

Colour observations are, of course, liable to many sources of error, but though observers may at different times receive different impressions from the same colour, I do not think it possible that any one accustomed to the use of colours would mistake yellow ochre for white.

In the case of *Jupiter*, means of comparison were never wanting: the light-belts N. and S. of the equatorial belt having, during the time of the change in the colour of this belt, always appeared nearly white.

I agree with Mr. Penrose that the colour of the equatorial belt appears paler than it did last year, but as the apparent diameter of the planet is smaller, as it is getting low in the heavens, and it now sets in twilight, I do not think much value should be attached to recent observations.

Mr. Ranyard informs me that he has found some observations of Schwabe on the colours of *Jupiter* in addition to the observations he had previously noted in his very interesting and useful paper on this subject published in the *Monthly Notices*.

On a New Spectroscopic Combination. By the Rev. Father Secchi.

(*Translation.*)

I have the honour to announce the discovery of a new spectroscopic combination by the aid of which it is possible to see the images of the solar spots and protuberances with the spectral rays; the whole in the same visual field. This result is arrived at in two different ways: 1°, by placing before the object-glass of the telescope a rather large prism, such as that which I use for the spectra of the stars, of six inches aperture; and by letting fall

on the slit of the ordinary spectroscope the coloured image produced at the focus of the telescope. 2°, the other less costly means consists in placing a direct-vision prism of great dispersive power in the course of the luminous rays before they arrive at the focus, and on the slit of the ordinary spectroscope.

By these two means we obtain in the field of the spectroscope the solar image formed of different coloured hues, with their spectral rays, but very precise and detailed, with the spots and edges very precise, so that it is possible to make out the details of the spots, as one would do with a coloured glass. If the solar edge falls near the ray C, which (as I have said) one sees at the same time with the image of the spots, one sees the chromosphere and protuberances mark themselves out like brilliant lines, more or less distant from the edge according to the height of the protuberances. The image of the spots is sharper as the slit is narrower. Thus one sees the protuberances as brilliant lines, but one only recognises their form by linear sections, as in Janssen's old method. It is, nevertheless, possible perfectly to make out and measure their height by measuring the distance of the (now brilliant) line C from the point of the solar boundary which is on the same normal therewith. One can also, by broadening the slit, make out the form of the protuberance, but there is then a loss of precision in the image. But keeping the slit narrow, one can make out with the greatest precision the height and place of the protuberances, and fix their position in respect to the spots and faculæ, in a much more simple and surer manner than with the ordinary spectroscope.

If in a spot there is a jet of hydrogen, one sees there the line C become brilliant, or at least diminish in blackness, and the calcium and iron lines grow broader, &c., as in the ordinary observations, but with greater satisfaction and accuracy of position.

Note on the use of Compound Prisms. By John Browning, Esq.

As some attention is being directed to the use of compound prisms, I think it desirable to place on record in the *Monthly Notices* the steps I have taken towards their introduction.

In 1864 Mr. Gassiot asked me to make for him a very powerful battery of bisulphide of carbon prisms.

Instead of making the sides of these prisms of plane and parallel glass, I cemented crown-glass prisms on to them in the manner shown in the diagram (fig. 1), where A represents the fluid prism, and B B the crown-glass prisms added on either side.

Fig. 1

This battery of prisms was described in the *Proceedings of*

the Royal Society, vol. xiii. p. 185, in a paper read by Mr. Gassiot, April 7th, 1864. From this paper I take the following extract:—

"In place of giving to the fluid prisms two pairs of parallel sides, advantage has been taken by Mr. Browning of the difference between the refractive and dispersive properties of crown glass and bisulphide of carbon; and prisms of crown glass, having a refracting angle of 6°, have been substituted for one of the outer plates of each prism, the bases of these crown-glass prisms being brought to correspond with the apex of the fluid prism.

"By this means the angle of minimum deviation of the prisms is so much altered that eleven prisms can be used instead of eight. An increase of dispersive power, due to refracting angles of 150° of the bisulphide of carbon, is thus gained, minus only the small amount of dispersion counteracted by the dispersive power of the crown-glass prisms in the contrary direction."

In July 1869, at the suggestion of Dr. Robinson, of Armagh, I made a large dense glass prism of 60° into a compound prism, the refracting angle of the dense prism being altered to 90°.

Since that time I have made several compound prisms of very dense flint glass and light crown glass, for the present Earl of Rosse.

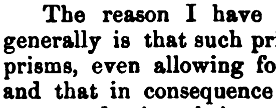
The reason I have not used such compound prisms more generally is that such prisms are more expensive than ordinary prisms, even allowing for the extra dispersive power obtained, and that in consequence of the minimum angle of deviation of compound prisms being greater, and their length greater, the size of spectroscopes would require to be increased, and they would thus be rendered more cumbersome, as well as more expensive.

A smaller number of compound prisms will produce a given amount of dispersion; but the number of prisms to be made is, under the most favourable circumstances, three instead of two; and in the case of a circular battery the smaller number of prisms will occupy a larger circle. The number of plane faces is also greater for a given amount of dispersion, the practical difficulties in securing accuracy not being diminished by the fact that four faces out of the six in a compound prism are connected together.

On the score of saving light such prisms possess undoubtedly some advantage.

After writing the above, Mr. Lewis Rutherford, of New York, has requested me to make him a compound prism of glass on a plan which seems to possess some advantages over any hitherto used. The diagram (fig. 2) represents this prism.

Fig. 2



In this diagram the two darkly shaded prisms are of dense flint of 90°, while the three other prisms are of crown. Such a prism is very nearly equal in dispersive power to three ordinary flint-glass prisms of 60°. There is no loss of light at the two intermediate surfaces, and it is much more compact.

I have found by experiment that the angles of the flint-glass prisms in this arangement cannot with advantage be made more than 90°, nor the outside crown-glass prisms less than 30°.

A Contrivance for a double Automatic Spectroscope, with compound Prisms on Mr. Grubb's plan. By Richard A. Proctor, B.A., Cambridge.

Mr. Browning having succeeded in constructing a double-battery spectroscope on the plan I submitted to the Society last December—with, however, a slight modification in the contrivance for automatic adjustment—I am encouraged to believe that the plan I am now about to describe will be found feasible; though certainly the difficulties which the optician will have to surmount are by no means slight.

I propose the substitution of Mr. Grubb's compound prism for the single prisms in my double battery, the single intermediate prism being replaced by an intermediate compound prism, shaped as shown in the accompanying view (fig. 1) of the complete

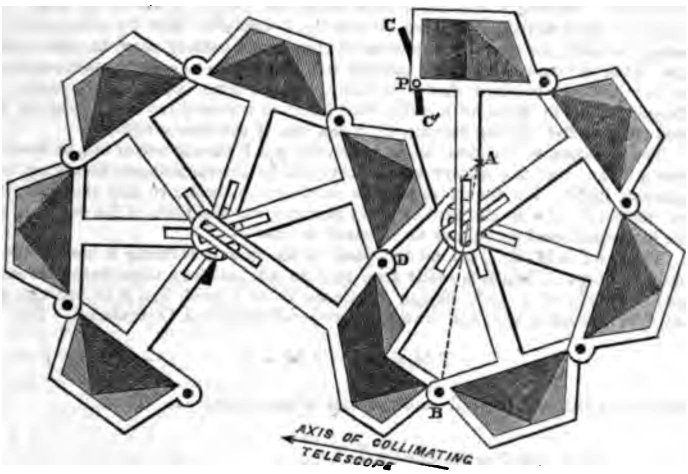


Fig. 1.

double battery. (The pivots on which the slots work have been omitted by the engraver.) This view, with the description given in my former paper serves to explain how the automatic adjustment is secured. But it will be noticed that the radial bar and the long slotted bar of the former description are removed. As I partly suspected when I suggested their use, they fail to communicate motion effectively to the second battery. The reason is easily seen: they work on that end of the second battery which

moves the most slowly, that is, where the leverage is least. When Mr. Browning found, on trial, that this was the case, another method—which, like the former, is theoretically exact—suggested itself to me. It is a property of the double battery that each prism of one battery has its base at right angles to some one prism of the other battery. Hence, if by means of a slotted T-square bar, this relation could be impressed on two corresponding prisms of the separate batteries, the adjustment would be mathematically exact. On trial, this method was found also to fail; and no other mathematically exact extension suggesting itself to me (and time pressing), I agreed that in the specimen double battery the last prism of the second battery should be carried by a simple radial motion making it travel as nearly as possible on its proper curve.*

* I should have preferred a cam, but Mr. Browning stated that there were practical objections. The theoretical advantage of a cam is, that the pivot p can be made to travel in the true curve, as C C′ (fig. 1). And in order to determine this curve, a construction founded on the plan first suggested by me could be adopted. Thus the dotted lines D A, B A show the position which the removed bars would take up. Now the motion of the first battery would bring these lines into position; and if then the slotted bars of the second battery were carried by means of their common pivot until this pivot was just centrally over the line A B, then the pin P would be in its proper place, which could readily be marked. Several marks being thus made, the proper figure of the cam C C′ would be determined, and more accurately, I conceive, than by endeavouring merely to make the amount of closing of the second battery equal to that of the first. That the curve C C′ is not circular will be obvious to every mathematician; nor is it difficult to show that it deviates enough from circularity to render it advisable that, in place of a radial movement, a trammel-movement should be used (to give the varying curvature), if the cam is absolutely rejected.

The equation to the *locus* along which the pin P should travel can be readily determined. But the *nature* of the curve can be shown without obtaining the equation itself. For the curve may be shown to be similar to that traversed by the last angle of a single battery of six prisms; and the form of the equation to this last-mentioned curve may be obtained at once—

Thus, let A B, B C, &c. be the bases of six prisms forming a single automatic battery. Then it is easily seen that in all positions these bases are successive chords of a circle having its centre O on a fixed line A O through A. Let P be the last angle, and draw P M perpendicular to A O produced. Put

$$\text{P M} = x \text{ and A M} = y;$$

and let the radius A O be R, and the base of each prism $2a$. Then,

$$\text{angle A O P} = 12 \sin^{-1}\left(\frac{a}{\text{R}}\right)$$

$$y = \text{R}\ (1 + \cos \text{P O M})$$

$$= \text{R}\left\{1 - \cos 12\left[\sin^{-1}\left(\frac{a}{\text{R}}\right)\right]\right\} \qquad \text{(i)}$$

$$\text{and } x = \text{R} \sin 12\left[\sin^{-1}\left(\frac{a}{\text{R}}\right)\right] \qquad \text{(ii)}$$

Eliminating R between (i) and (ii), we shall obtain the equation to the *locus of* P, and the curve will clearly be of a very high order. It is, in fact, a some-

The proper shape for the intermediate prism is easily deduced. For let A D C B (fig. 3) be the half of one of Mr. Grubb's compound prisms. Then the light which is passing parallel to A B must be totally reflected by a face B E inclined at an angle of 45° to its course. We must therefore take B C E a half square. Then, that the exit of the light may be on a course exactly similar to its entrance, we must add the half of a compound prism C E G F exactly similar to B C D A. Thus the construction for determining the section A C F E B of the flint portion of the intermediate prism is obtained very readily by sole reference to the figure of half a compound prism.

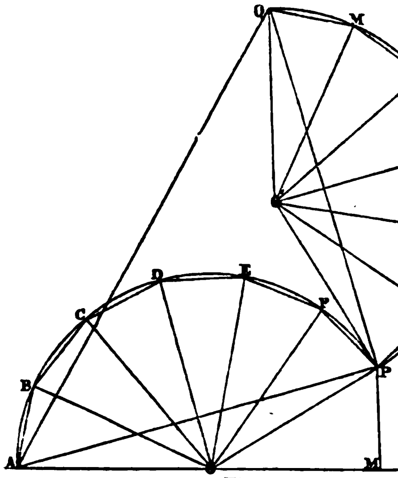
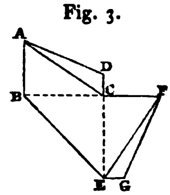


Fig. 2.

Fig. 3.



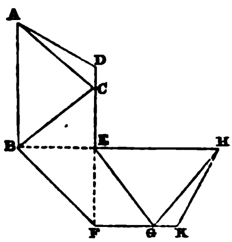


Fig. 4.

Note also that, if a symmetrical compound prism consist of more parts than three, a similar construction will give the proper shape for an intermediate compound prism in a double battery of such prisms. Thus, let A D C E B (fig. 4) be one half of a compound prism formed of five prisms; then the construction given in fig. 4 shows the proper section for an intermediate prism. B E F is half a square, and the portion E F G K H is exactly similar to the half-compound prism A D C E B.

The battery pictured in fig. 1 would give about twice the dispersion given by my double battery of single prisms: in other

what peculiar spiral. Now, if we add a second battery of the same number of prisms (an intermediate prism giving one total reflexion), we obtain the base lines P G, G H, &c., the centre O′ of the circle on which they lie being a tangent to the circle A D P. And it is easily seen that the last angle Q will always be so placed that A P Q is half a square. Hence the locus of Q is precisely similar to the *locus* of P, but enlarged in the proportion of $\sqrt{2}$ to 1, and tilted round A in the positive direction of angle-measurement, through 45 degrees.

words, it would give a dispersion equal to that given by 36 equilateral prisms of heavy flint glass. It will be understood that the light is taken twice through the double battery, returning at a higher level, after exactly the same fashion as in the battery described by Mr. Grubb in the December number of the *Monthly Notices.*

Observations of Mars, with Drawings.
By John Joynson, Esq.

The accompanying drawings of the planet *Mars*, as it has been seen during the late opposition, are arranged so as to exhibit its aspect at regular intervals of thirty-seven minutes. There are, in consequence, four more in number than those in the sets already sent to the Society for the oppositions of 1862, 1864, and 1867; but they can be easily compared with them, as there has been but little change in the appearance of the planet since the last opposition.

It will be noticed that the aspect of the planet is entirely changed in about ten diagrams, or about every six hours, showing that we can only see about one-fourth of the planet, or about half the true disk at one time; about one-eighth on each limb is hidden by the rotundity of the planet. This is confirmed by the fact that the aspect changes slower at the neighbourhood of the limbs than the same part does when it gets about the centre of the disk. The present arrangement enables any one to see at once what parts are on opposite meridians, as one hemisphere is represented by twenty diagrams.

The planet has been apparently tilted over, so as to bring the North Pole more directly towards us, though the effect is more perceptible on the southern limb than about the pole itself. The North Polar snow has been of much smaller extent than it was at the last opposition, and has had very much the appearance of that at the South Pole, as seen in 1862. It has not been quite on the limb, as it has appeared somewhat larger on one side than on the opposite: though it has sometimes, even on both, appeared perfectly round, owing to its smallness and brightness. There is only one other remark to make as to any apparent change on the disk, and that is, that the watery projection from the North Polar snows seen about the 22nd March has been much larger than at the last opposition; indeed the whole disk about the pole has been darker than previously.

The general colour of the disk (saving the band and the channel from it to the circumpolar waters) has been yellowish, with a brownish tinge towards the pole. But it is difficult to say what the real colour is, for the same part of the disk has varied from night to night from dark-yellow to very nearly white, according to the state of our own atmosphere.

These drawings, in conjunction with those previously sent, prove beyond doubt that the "band" and "wine-glass shaped channel" from it, are permanent features of the planet, and that any apparent change in them arises from the various aspects that are presented by the planet itself, as seen from the Earth.

In addition to the above set of drawings, I send copies of those already sent for the oppositions in 1862, 1864, and 1867, arranged so that the corresponding drawing for each appears under that for the previous opposition, thus showing the apparent changes, and facilitating their comparison.

Waterloo, near Liverpool, 10 *May,* 1871.

On a Free-Regulator Clock. By Sidney B. Kincaid, Esq.

The common pendulum-clock is essentially a very imperfect adaptation of theory to practice, because, while the theory contemplates a body swinging under the action of a *constant* force, in practice it is influenced by forces so various that it is only with the utmost difficulty that a clever workman can render their sum approximately constant in a clock of moderate size, and when the requirement that it should drive a pair of large hands on a weather-dial necessitates construction on a scale belonging rather to the province of the engineer, it is deemed indeed a triumph of skill to produce a timepiece which can be depended on as such for any lengthened period without frequent correction by the transit-instrument.

I would now submit to the Royal Astronomical Society a plan in which, by making the regulator mechanically free of the going and recording apparatus, the difficulty of construction I have referred to is obviated, inasmuch as a slight want of care and accuracy in the finish of these latter parts of the clock does not tend to impair the exactness of its performance.

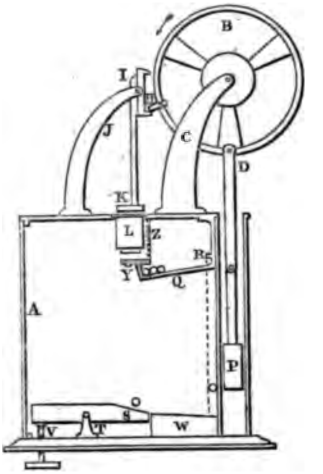
I propose, as will be seen, to substitute for the pendulum as the controlling agency, iron spheres falling *in vacuo*, and to record the falls and replace the spheres by means of a galvanic current. No doubt some unaccustomed to its use may feel prejudiced against the employment of electricity, but with a self-amalgamating Smee, or a constant, battery, there need be no much trouble about it.

In the diagram:—

A, is the case, exhausted of air to prevent unequal resistances due to changes of temperature.

B, a wheel (C its support) on the axis of which and connected to it is a pinion, such that if $\frac{1}{t}$ be the fraction of a second occupied by the falling of a ball, 60 t turns of B make one turn of seconds-wheel of clock. To the edge of B is fixed by a loose pin,

D, a rod carrying the permanent magnet P, which slides up and down against outside of case.



H, is the pin of C, which engages the escapement I (J its support) to which is attached the armature K acted on by the electro-magnet,

L, within the case. *Inside* the case are also,—

Y, the release-piece with its spring Z:

O, a lever turning on centre T and resting in its normal position on the screw V, by which the circuit is closed between battery and magnet:

W, the lower inclined plane.

R, a stop just above Q, and,

Q, the upper incline.

Y is supported by two standards, not shown in diagram, constructed on principle of gridiron pendulum, so that its height may be the same at all temperatures. For the sake of clearness the electrical connexions are also omitted.

The action of the apparatus is as follows:—

A ball having fallen, immediately that it meets S, it breaks contact, the piece Y tilting sideways then instantly disengages another ball, and the escapement armature K no longer being held by the magnet, allows the wheel B to be turned once by the clock train, this raises the magnet P, drawing up the ball on W until it meets the stop R, which releases it from the magnet, and leaves it to roll against a stop-pin and against Y, and lets (P) down again; meanwhile the ball which fell has rolled down S and W into position to be picked up in its turn, and S being relieved of its weight, contact is restored; when the next ball strikes S the same movements take place. A guard-pin prevents more than one ball being liberated at once by Y. Of course L must be so placed as not to act directly on the balls.

Trinity College, Cambridge, May 6, 1871.

On η Argûs. By J. Tebbutt, Esq.

The accompanying table exhibits the results of comparisons of *η Argûs* made by me during the period 1854–1870. The magnitudes of the comparison stars employed down to the end of 1863, have been taken from Tables A and C on pp. 334, 341, of Sir J. Herschel's *Results of Astronomical Observations at the Cape of Good Hope*, 1834–8. For all the observations since

that year magnitudes have been assigned to the comparison stars in accordance with my own estimates. I have divided the series of observations into groups, each group having concluded magnitudes nearly agreeing with one another. The mean date and magnitude for each group have then been determined. On projecting these results in a curve, it will be seen that during the past three years there have been small annual fluctuations in the light of the star. Whether the curve represents with any approach to accuracy the real fluctuations in the light of the Variable must, of course, depend on the value to be attached to my judgment in the comparison observations. Of this, however, I am quite certain, that η *Argûs* has been alternately above and below a mean magnitude during the past three or four years, and that the limit of these fluctuations from maximum to minimum is about a quarter of a magnitude. For the past three years the star has been invisible to the naked eye.

In conclusion, I may state that of the stars contained in the Catalogue on p. 42 of the work before referred to those numbered 387 and 522 are very nearly equal, the latter being somewhat the brighter. These stars are marked in the Catalogue as of the 6th and 7th magnitude respectively. 1203 is considerably brighter than 1183, although the catalogue gives 7 and 6 as their respective magnitudes.

	Dates of the Comparisons.	Mean Date of Comparison Year	Day	Mean of concluded Mags.
1854	July 5	1854	186	1·10
1860	May 4, 18	1860	132	3·41
1862	Jan. 23, 26; June 23; July 3, 4, 7, 10, 11, 15, 16, 17, 18, 22, 24, 25, 26; Aug. 22, 27, 28	1862	184	4·57
1863	April 15, 16; May 24	1863	118	4·66
1864	March 23; April 23; May 6, 11, 31; July 28	1864	136	5·13
1865	Feb. 22, 23, 24; March 14	1865	59	5·25
1866	Feb. 16; March 22; June 29; July 2, 4	1866	135	5·52
1866	Dec. 3, 8, 11, 12. 1867, Jan. 12, 26; March 2; April 17; May 4; July 27	1867	45	6·04
1867	Dec. 28, 31	1867	364	6·35
1868	Feb. 26, 27	1868	58	6·20
1868	April 13, 22, 29; May 9, 16, 19, 22, 26; June 12; July 21, 31; August 24; Dec. 17	1868	169	6·30
1869	Feb. 8, 18, 23	1869	47	6·20
1869	March 10; April 19; May 19	1869	106	6·32
1869	May 26; June 25; July 23; August 10; Sept. 6, 21; Dec. 6, 25. 1870, Jan. 8, 13	1869	271	6·39
1870	Jan. 22, 28; Feb. 8, 25; March 25, 26	1870	52	6·25
1870	May 7; June 3, 6, 7, 16; July 4; August 20, 25; Sept. 26, 27	1870	196	6·40
1870	Nov. 28; Dec. 6, 29	1870	345	6·25

Windsor, N. S. Wales, February 23, 18 1

On the Total Solar Eclipse of 1878, *July* 29.
By J. R. Hind, F.R.S.

The following particulars relating to this eclipse may be of interest in anticipation of the appearance of the various ephemerides for the year. They are founded upon the tables of Damoiseau and Carlini, but will afford a pretty close approximation to the actual circumstances of the eclipse. No doubt every possible use will be made of this phenomenon by the astronomers of the United States.

Elements.

G.M.T. of Conjunction in R.A. 1878, July 29, $9^h\ 22^m\ 11^s.7$

R.A. of Sun and Moon	..	128° 56′ 4″.5
Hor. Mot. in R.A.	Sun	2 26.7
,,	Moon	35 59.3
North Declination	Sun	18 39 3.2
,,	Moon	19 18 33.3
Hor. Mot. in Decl.	Sun	— 0 35.9
,,	Moon	— 12 9.8
Hor. Parallax	Sun	8.8
,,	Moon	59 36.7
Semi-diameter	Sun	15 47.7
,,	Moon	16 16.3

The following points will indicate the course of the central eclipse:—

Central beginning	117° 48′ E	54° 17 N
	178 55 E	65 57
	142 52 W	61 58
Sun on Meridian	139 0	60 32
	135 50	59 31
	132 29	58 9
	123 31	53 19
	116 43	49 16
	110 26	44 20
	97 7	33 4
	90 49	28 23
	82 29	23 19
Central ending	69 31 W	16 56 N

Special calculations for various points near the line of central eclipse:—

(1). For 135° 50′ W., 59° 31′ N., the Sun nearly on the Meridian.

	h m s	
Totality begins	0 21 29·7	Local Mean Time.
,, ends	0 24 35·8	
∴ Duration	3 6·1	
Sun's Altitude	50°	

(2). For Denver, Colorado.

	h m s	
Totality begins	3 29 0·3	Denver M.T.
,, ends	3 31 46·8	
∴ Duration	2 46·5	
Sun's Altitude	42°	

(3). For Havannah, Cuba.

	h m s	
Totality begins	5 34 31·8	Havannah M.T.
,, ends	5 36 25·2	
∴ Duration	1 53·4	
Sun's Altitude	16°	

(4). For Port-au-Prince, Hayti.

	h m s	
Totality begins	6 18 46·1	Port-au-Prince M.T.
,, ends	6 20 10·1	
∴ Duration	1 24·0	
Sun's Altitude	4°	

The greatest duration of totality on the central line will be about $3^m\ 8^s$, and the mean semidiameter of the zone of totality in the United States about 51′.

I have carefully examined every part of the above calculation, a very necessary precaution when eclipse-work is undertaken by a single computer.

The Occultation of Uranus on Thursday, March 2, 1871. By J. Maguire, Esq.

In the last Number of the *Monthly Notices* Captain Wm. Noble states a circumstance which leads him to doubt the accuracy of the time of disappearance of *Uranus*, as shown in page 452 of the *Nautical Almanac*. He says that returning at $14^h\ 5^m$ sidereal time to his Equatoreal which had been previously set upon *Uranus*, he found that the planet *had disappeared* two minutes before the predicted time. The times are as follows :—

	S.T. h m	M.T. h m
Disappearance	14 7	15 25
Reappearance..................	15 0	16 19

I have made a calculation of the time of disappearance which I find to be $15^h\ 24^m\ 43^s$, and this agrees well with the $15^h\ 25^m$ of the *Nautical Almanac*, which rejects in the list of Occultations all subdivisions of the minute; but I find there is a difference in the time of reappearance—my calculation being $16^h\ 18^m\ 9^s$ against the *Nautical Almanac* $16^h\ 19^m$, which it may be noticed is not the equivalent for $15^h\ 0^m$ sidereal time.

If Captain Noble's observation was made at Maresfield, the mean time of disappearance there would be about $15^h\ 25^m\ 44^s$.

But how is the observation to be reconciled with the calculated time? Must we conclude that the tabulated places of the Moon or *Uranus* in the *Nautical Almanac* are wrong*—or with respect to the observation itself, may we not entertain a doubt as to the presence of some of the conditions which are essential to a good determination.

Norwich, 26 *April*, 1871.

On the First Comet of 1867. By J. R. Hind, F.R.S.

In No. 1639 of the *Astronomische Nachrichten*, Professor Winlock has given elliptic elements of the comet discovered at Marseilles by M. Stephan, in January 1867, calculated by Mr. Searle, of Harvard Observatory, Cambridge, U. S., in which the period of revolution is 33·62 years. These elements are stated to represent closely the observations taken with the great refractor of that observatory; and those who have had occasion to use the Harvard observations will be aware that they possess an extreme precision.

I have remarked in this orbit a very near approach to that of the planet *Uranus*, when the comet passes its descending node (5789 days before perihelion). At this point the comet's heliocentric ecliptical co-ordinates are

$$x = -3·784730 \qquad y = -18·763121 \qquad z = 0$$

and in this longitude the similar co-ordinates of *Uranus* are

$$x = -3·784974 \qquad y = -18·764332 \qquad z = -0·023115$$

whence the distance between the two orbits is 0·02343 only; an approximation which appears to explain the cause of the actual form of the comet's orbit.

* It was noticed at the Meeting, that the existing Tables of *Uranus* (Bouvard's), from which the *Nautical Almanac* places are calculated, are confessedly inaccurate to an extent which quite accounts for the error.—Ed.

It is also to be remarked that there is a singular commensurability in the periods

$$5 \times \text{sidereal period of Comet} = 168^{\text{yrs.}}\!\cdot 120$$
$$2 \times \text{sidereal period of } Uranus = 168\cdot 032$$

affording a parallel to the well-known 5 to 2 relation in the great inequality of *Jupiter* and *Saturn*.

If Mr. Searle's elements are nearly correct (as appears to be the case), the comet would not suffer any extraordinary perturbation about the nodal passage in 1817, the nearest approach to *Uranus* being then about 1·05; but in 1649 the distance between the two bodies may have been much less.

Ephemeris of the Periodical Comet of Tuttle, for its approaching Reappearance. By J. R. Hind, F.R.S.

In No. 1840 of the *Astronomische Nachrichten*, Prof. Luther has published an orbit of this comet found amongst the papers of the late Dr. Tischler, of Königsberg, in which the perturbations during the present revolution had been partially included. According to these elements the next perihelion passage will occur about 1871, Nov. 30·5, and on this assumption I have calculated the following ephemeris to facilitate the comet's rediscovery. The expressions for the comet's heliocentric co-ordinates referred to the Mean Equinox of 1871·0 are

$$x = r\,.\,[9\cdot 7663098]\,.\,\sin(v + 205^\circ\,35'\,31''),$$
$$y = r\,.\,[9\cdot 9888950]\,.\,\sin(v + 97\ \ 1\ \ 10),$$
$$z = r\,.\,[9\cdot 9253116)\,.\,\sin(v + 178\ \ 34\ \ 48).$$

Ephemeris for Greenwich Noon.

T = 1871, Nov. 30·5.

1871.	R.A.	Decl.	Log Δ.	Log r.	$\frac{1}{r^2\,\Delta^2}$
	° ′	° ′			
Sept. 1	100 13·2	+62 22·7	0·2377	0·2201	0·12
3	102 30·5	61 56·4			
5	104 45·0	61 27·4	0·2221	0·2094	0·14
7	106 56·6	60 55·6			
9	109 5·3	60 21·1	0·2059	0·1984	0·16
11	111 10·9	59 43·8			
13	113 13·4	59 3·7	0·1890	0·1873	0·18
15	115 12·8	58 20·7			
17	117 9·1	57 34·9	0·1713	0·1760	0·20
19	119 2·1	56 46·2			
21	120 52·0	55 54·5	0·1628	0·1645	0·23
23	122 38·8	54 59·8			

1871.	R.A.	Decl.	Log Δ.	Log r.	$\frac{1}{r^2 \Delta^2}$
	° ′	° ′			
Sept. 25	124 22·5	54 2·1	0·1335	0·1530	0·27
27	126 3·1	53 1·3			
29	127 40·8	51 57·2	0·1134	0·1413	0·31
Oct. 1	129 15·7	50 49·5			
3	130 47·8	49 37·9	0·0924	0·1297	0·36
5	132 17·3	48 22·4			
7	133 44·3	47 2·6	0·0706	0·1180	0·42
9	135 8·9	45 38·4			
11	136 31·3	44 9·4	0·0479	0·1065	0·49
13	137 51·5	42 35·5			
15	139 9·7	40 56·3	0·0245	0·0951	0·58
17	140 25·9	39 11·4			
19	141 40·0	+37 20·8	0·0003	0·0839	0·68

The comet's track in the heavens would, therefore, appear to be very favourable for observation in these latitudes.

Note on the Re-appearance of Encke's Comet in 1871. By J. R. Hind, F.R.S.

The approaching re-appearance of the Comet of Encke is likely to take place under nearly the most favourable circumstances possible for observation in the Northern hemisphere. Complete ephemerides will no doubt be issued from Berlin in due time. Meanwhile the following places may serve to indicate its earlier position in the heavens. They are computed on the assumption that the perihelion passage takes place in 1871, Dec. 29·0, which is about the time assigned by Prof. Förster's elements for the last return in 1868, neglecting perturbations.

0^h G.M.T.	R.A.	Decl.	Log. Δ.
	° ′	° ′	
Aug. 21	31 11	+24 4	0·1929
31	31 48	26 0	0·1284
Sept. 10	31 32	28 8	0·0561
20	29 57	30 32	9·9750
30	26 18	33 13	9·8842
Oct. 10	19 19	+36 4	9·7841

On De Vico's Comet of Short Period. By J. R. Hind, F.R.S.

It is well known that the periodical comet detected by De Vico at Rome in 1844, August 22, has not been observed at any one of its returns since that year. In 1850 its position in the

heavens was very unfavourable, and in 1855, when observations were expected, possibly from the prevalence of clouded skies, it escaped notice again. Nor at its returns in 1860 and 1866 was it detected, though in the former year it was sought for with the great refractor of the Harvard Observatory, and in the latter year I circulated a sweeping Ephemeris. Another return to perihelion must be approaching, but from the elaborate investigations of Prof. Brünnow, there appears to be so much uncertainty attaching to the value of the mean diurnal motion in 1844, notwithstanding the fine series of observations made by Mr. Otto Struve at Poulkova, that the date of next perihelion passage can hardly be assigned with any degree of confidence within several months. We must, therefore, in all probability, depend upon the success of those who occupy themselves in sweeping the heavens for telescopic comets, to recover it once more. (See Professor Brünnow's remarks in the Ann Arbor *Astronomical Notices*, No. 3.)

The following values of the comet's heliocentric equatorial coordinates from 130 days before, to 90 days after perihelion, may perhaps assist in identifying it, should an accidental discovery be made:—

Days from Perihelion.	*x*	*y*	*z*	Log. *r*.
−130	−0·5221	−1·6640	−0·7401	0·27746
120	0·3791	1·6221	0·7280	0·25953
110	0·2343	1·5723	0·7123	0·24099
100	−0·0881	1·5136	0·6927	0·22185
90	+0·0586	1·4452	0·6686	0·20228
80	0·2048	1·3663	0·6396	0·18248
70	0·3493	1·2759	0·6053	0·16272
60	0·4904	1·1732	0·5651	0·14344
50	0·6261	1·0577	0·5187	0·12513
40	0·7540	0·9290	0·4659	0·10850
30	0·8713	0·7874	0·4066	0·09434
20	0·9752	0·6338	0·3411	0·08348
−10	1·0628	0·4696	0·2699	0·07654
0	1·1323	0·2974	0·1941	0·07428
+10	1·1812	−0·1198	0·1148	
20	1·2101	+0·0598	−0·0335	
30	1·2187	0·2384	+0·0483	
40	1·2086	0·4135	0·1295	
50	1·1817	0·5828	0·2088	
60	1·1401	0·7450	0·2856	
70	1·0859	0·8989	0·3592	
80	1·0212	1·0442	0·4294	
+90	+0·9480	+1·1807	+0·4960	

In computing the above co-ordinates the values of π, ☊, and

B

i in Prof. Brünnow's orbit for 1855 (*Mémoire sur la Comète Elliptique de De Vico*) were brought up to 1872.0 and adopted.

I may here remark that the time of the next return of Peters' periodical Comet is much more uncertain than that of De Vico's, and the only chance of recovering it will be in the manner I have alluded to above. The last calculations of Dr. Peters with reference to this comet are those detailed in Prof. Brünnow's *Astronomical Notices*, as I learned in conversation with Dr. Peters last December.

Elements and Ephemeris of the First Comet of 1871, *discovered by Dr. Winnecke on April* 7. By J. R. Hind, F.R.S.

From Dr. Winnecke's observation on April 7th and two at Mr. Bishop's Observatory on the 12th and 19th I have calculated the following parabolic elements, taking all the small corrections into account:—

Perihelion Passage, 1871, June 10·9958, G.M.T.

Longitude of Perihelion	139° 43′ 19″	Mean Equinox 1871·0.
Longitude of Ascending Node ..	278 40 45	
Inclination	87 54 10	
Log. Perihelion distance........	9·8292565.	

Motion direct.

$$x = r \,.\, [9{\cdot}19083] \,.\, \sin (v + 297^\circ\ 33'{\cdot}7),$$
$$y = r \,.\, [9{\cdot}99486] \,.\, \sin (v + 107\ \ 37{\cdot}7),$$
$$z = r \,.\, [9{\cdot}99985] \,.\, \sin (v + 197\ \ 51{\cdot}7).$$

The following Ephemeris shows that there is a probability that the comet may be observed after the perihelion passage, if sought for at the Southern observatories:—

At Greenwich Noon.

1871.	R.A.	Decl.	Log. Δ.	Log. r.	$\frac{1}{r^2 . \Delta^2}$
	° ′	° ′			
June 24	94 11	− 5 50	0·1509	9·8616	0·94
28	96 44	10 1	0·1356	9·8816	
July 2	99 33	14 21	0·1197	9·9041	0·90
6	102 42	18 50	0·1043	9·9281	
10	106 17	23 28	0·0900	9·9527	0·82
14	110 23	28 14	0·0775	9·9774	
18	115 7	33 5	0·0673	0·0017	0·73
22	120 36	37 54	0·0606	0·0255	
26	126 59	42 32	0·0580	0·0484	0·61
30	134 23	−46 50	0·0595	0·0705	

Protocolle der Verhandlungen der zweiten Conferenz der Commission für die Vorberathung der Beobachtung des Venusdurchgangs von 1874. Berlin, 27–28 Marz, 1871.

The commissioners present were Hansen, Argelander, Paschen, Seidel, Schönfeld, Bruhns, Förster, Rümker, Auwers, and Winnecke. It was resolved that there should be four heliometer-stations, viz. in the northern hemisphere, one in Japan or China (it being assumed that there would be a Russian heliometer-expedition or expeditions to north-eastern Asia), and in the southern hemisphere three, viz. two in the neighbourhoods of Kerguelen's Island and Auckland's Islands, and the third at Mauritius. And four photographic stations, viz. the northern heliometer-station, those of Kerguelen's and the Auckland Islands, and a station in Persia between Mascat and Teheran. It was considered that there would be required nine astronomers, eight photographers, and nine assistants.

Ueber das Rotations-gesetz der Sonne und der grossen Planeten, von F. Zöllner (from the Berichten of the R. Saxon Society of Sciences, Feb. 1871, *pp.* 49–113.)

The daily rotation-angle of a point on the Sun's surface, as determined by observations of the spots, is found to vary according to the latitude ϕ of the point on the Sun's surface, and Mr. Carrington obtained for this angle the empirical value

$$\xi = 865' - 165' \sin^{\frac{7}{4}}\phi,$$

but for this M. Faye proposed to substitute,

$$\xi = 862' - 186' \sin^2\phi,$$

The author, making certain suppositions as to the fluid or gaseous envelope of the Sun, arrives at an expression of the form,

$$\xi = \frac{A - B \sin^2\phi}{\cos\phi}$$

and he finds from the observations

For North Hemisphere	For South Hemisphere
$A = 863'{\cdot}8$	$A = 861'{\cdot}8$
$B = 613{\cdot}2$	$B = 620{\cdot}2$

or assuming for A in the two hemispheres the common value $=863'{\cdot}8$, then the values for B in the two hemispheres are $=613'{\cdot}2$ and $631'{\cdot}1$ respectively. He finds that his formulæ represent the observations somewhat better than either of the

former ones (but the difference is by no means striking). He examines also in connexion with observations by Spörer the formula

$$\xi = \frac{A' - B' \sin^2\phi}{\cos\phi} - C' \sin\phi$$

with the values

$$A' = 877'{\cdot}07$$
$$B' = 387{\cdot}07$$
$$C' = 154{\cdot}39$$

The memoir contains also a discussion of the evidence afforded by the observations of *Jupiter* and *Saturn* of a like drift of the exterior envelopes over the surfaces of the planets, and various discussions as to the phenomena generally.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXI. *June* 9, 1871. No. 8.

William Lassell, Esq., F.R.S., President, in the Chair.

John Brett, Esq., 6 Pump Court, Temple;
Rev. Reginald F. Dale, M.A., Queen's College, Oxford; and
Rev. Archibald Robertson, Burrage Road, Plumstead,

were balloted for, and duly elected Fellows of the Society.

The Variations of Gravity in the Western Provinces of Russia. By A. Sawitsch. (Abstract.)

A great arc of the meridian having been measured in Russia with all the precision which modern methods of observation will admit of, it became an interesting subject to examine the variations of the intensity of gravity in the districts traversed by this arc, and to compare the progress of those changes with the variations which are observed in the direction of gravity determined at several stations by astronomical observations and geodetical operations. An extensive series of pendulum observations was therefore arranged by the Academy of Sciences of St. Petersburg, to be made at certain stations between Tornea in Finland, and Ismail in Moldavia, selecting only those points of which the geographical positions and elevations above the mean level of the sea were determined in connexion with the great arc of meridian. The observations between Tornea and St. Petersburg were made during the summer of 1865 by M. Sawitsch and M. Lenz; those between St. Petersburg and Ismail were made in 1866 and 1868 by M. Sawitsch and M. Smyslof.

Two reversible pendulums were placed at the service of the observers. They were constructed by M. Repsold, of Hamburg. The pendulums differed from the English instruments both in their dimensions and in the arrangements of some of their parts. During the experiments, the clock and its tripod were inclosed in a case so that currents of air had no effect on the oscillations of the pendulum. A standard scale for the examination of the distance between the two knife-edges was also constructed by M. Repsold, and verified by M. Smyslof by comparison with the normal standard constructed by Messrs. Troughton and Simms, now deposited at the Observatory of Pulkowa.

M. Brauer, of St. Petersburg, furnished an apparatus for determining the centre of gravity of the pendulum, and also for the examinations of the parallelism of the knife-edges. M. Sawitsch remarks that these pendulums have now been placed at the disposal of Colonel Walker, Superintendent of the Great Trigonometrical Survey of India.

The observation of the duration of the oscillations of the pendulum consisted in noting the time of the coincidences of the pendulum and clock, the daily rate of the latter being deduced from comparisons of it with chronometers. A transit-instrument was erected on a brick pier, and the daily rate of the chronometers determined from transits of stars. Three or four coincidences and the temperature were observed at the beginning, in the middle, and at the end of each "swing." Three thermometers were placed in the interior of the pendulum-case, one at the upper end, another near the centre, and the third at the bottom end of the pendulum. The barometer was read at the beginning and end of each "swing."

M. Sawitsch gives in detail the formulæ used by him in the reductions, and also the results of the observations, which are given for twelve different stations situated between 65° 51′ and 45° 20′ north latitude. At St. Petersburg the length of the seconds' pendulum found directly from the observations is 440·958 Paris lines, the latitude being 59° 56′ 30″.

"Our object was not so much to determine the absolute length of the pendulum as to collect new data on the variations of gravity, and to compare them with those which have been found at other stations on the terrestrial surface. We know that at London the length of the simple seconds' pendulum has been determined with great precision by Capt. Kater and by General Sir E. Sabine; and that the measures made in Great Britain by M. Biot agree perfectly with those obtained by the English *savants*, while those made in France by the English *savants* give the same results as those of M. Biot. To these determinations we may also add the observations made by travellers and naval officers in different parts of the world. To connect our experiments with the preceding system of researches, without a breaking of continuity, we prefer not to adopt our direct determination of the length of the pendulum at St. Petersburg,

but rather that which was deduced from the oscillations of a similar invariable pendulum, observed by M. le Comte Luetke at St. Petersburg and at the Royal Observatory at Greenwich; the difference between the length of the seconds' pendulum at Greenwich and London having been accurately determined by Sir E. Sabine. Thus the length of the simple seconds' pendulum being known at London, we can calculate its length at St. Petersburg according to the relation of the squares of the numbers of infinitely small oscillations which the comparison pendulum, reduced to the same temperature *in vacuo*, has made at each of the stations in a mean day. In this manner, the calculation gives for the length of the simple seconds' pendulum at St. Petersburg 39·16975 English inches, or 441·0319 Paris lines. Assuming this length, we have deduced, as the result of our observations, the values contained in the following table."

Place of Observation.	Latitude N.	Longitude E. from Greenwich.	Length of the Seconds' Pendulum in Paris lines.
	° ′ ″	h m s	
Tornea	65 50 43	1 36 54	441·2525
Nicolaistadt	63 5 33	1 26 26	441·1293
St. Petersburg	59 56 30	2 1 14	441·0319
Réval	59 26 37	1 39 1	441·0190
Dorpat	58 22 47	1 46 54	440·9762
Jacobstadt	56 30 3	1 43 4	440·8900
Wilna	54 41 2	1 41 12	440·8353
Bélin	52 2 22	1 40 52	440·7268
Kréménetz	50 6 8	1 42 54	440·6533
Kaménetz-Podolsk	48 4 39	1 46 18	440·5844
Kischinef	47 1 30	1 55 18	440·5278
Ismail	45 20 34	1 55 16	440·4479

To examine the accuracy of each of the results, M. Sawitsch has compared the length of the pendulum observed at each station with the corresponding length obtained from the formulæ given in his paper. The residual errors are as follows: the sign + denoting that the observed length is greater than the calculated length.

	Paris Lines.		Paris Lines.
Tornea	+0·0200	Wilna	−0·0001
Nicolaistadt	−0·0141	Bélin	−0·0035
St. Petersburg	−0·0017	Kréménetz	+0·0017
Réval	+0·0033	Kaménetz-Podolsk	+0·0160
Dorpat	−0·0002	Kischinef	+0·0030
Jacobstadt	−0·0157	Ismail	−0·0071

The sum of the positive residual errors is +0·0440, and of the negative residual errors −0·0423.

Thus the formula agrees well with the equations of condition. With regard to individual errors, they depend upon errors of observation and upon anomalies in the intensities of terrestrial gravity; but it is difficult to discover in the differences given above any certain traces of these anomalies and of the local causes which produce them.

"In the work of W. Struve on the arc of meridian between the Danube and the Arctic Sea is a detailed discussion of the latitudes of the principal places between the North Cape and the Danube. The differences in latitude found directly from the astronomical observations vary only $\pm 1''\cdot 75$ from those deduced from the geodetical operations. Although these differences are very much larger than the errors of observation, they are not as great as those which may be found in corresponding operations in other countries. Our stations are in the neighbourhood of the places discussed by M. Struve; so that it appears that in the great plains of Western Russia the directions and intensities of gravity are not subject to anomalies which change sensibly from one of our stations to another."

On the Physical Changes in Jupiter. By A. C. Ranyard, Esq.

It was suggested at one of the recent meetings of the Society, that if *Jupiter* had at any time been studied with sufficient optical power the same details would always have been apparent, and it was hinted that much which individual observers had been disposed to record as changes occurring upon the surface of *Jupiter*, was really to be attributed to the increased power of the instruments used.

In answer to such objections I propose to compare a small series of drawings, all made within the last twenty years, by well-known observers and with large instruments.

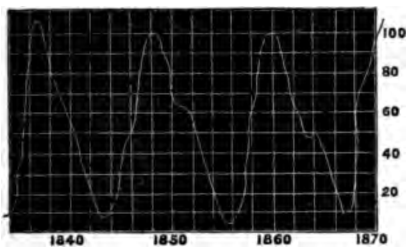
We will commence with a drawing by Mr. Lassell, a very poor woodcut from which is given at page 134 of vol. x. of the *Monthly Notices*. It was taken with his 20-feet reflector and 24 inches aperture. *Jupiter* is there represented within two years of the great Sun-spot maximum of 1848, and the strongly-marked white spots in the southern belts, and the broken condition of the equatorial region, are very apparent.

Within a year of the next Sun-spot minimum Mr. W. De La Rue made his large and well-known drawing of *Jupiter* with a 13-inch silver speculum. Its date is October 1856, and his engraving, which was largely circulated, was at that time universally acknowledged to be a very just representation of the state of the planet. It is full of the smallest details in the belts, yet there are no traces to be found of Dawes' markings, bright points, northern or southern eggs, or equatorial port-holes.

A second drawing, made in 1856 by Piazzi Smyth on *Teneriffe* with a $7\frac{1}{4}$-inch refractor, when *Jupiter* was near the zenith,

entirely endorses that of Mr. W. De La Rue. The next Sun-spot maximum occurred at the end of 1859; and in Nov. 1858, Mr. Lassell again noticed and figured the Dawes' markings and white spots. He says, at page 52 of the *Monthly Notices*, vol. xix., "One of the most novel features I noticed was the existence of a numerous group of white spots in the bright equatorial region, far more delicate and difficult to see than those in the southern hemisphere, which I first noticed in March 1850. For several years I failed to see any such spots upon the face of *Jupiter* at all, but last year they appeared again in the same quarter of the planet, and were attentively and ably observed by Mr. Dawes."

In September 1859 the flocculent cloudy port-holes in the principal belts made their appearance and were beautifully drawn by Sir J. Keith Murray with his 9-inch refractor. They remained more or less a marked feature of the central belt till the end of 1861, and were figured by many observers with telescopes varying from 5 inches in aperture and upwards — though they are not to be found in a very elaborate series of drawings made by Mr. Gorton with a $3\frac{1}{2}$-inch Cook in the years 1860–1861.



I cannot here omit to quote a remark made by the Astronomer Royal in his Report to the Visitors of the Greenwich Observatory for the year 1860–61. Speaking of the great Equatoreal, he says, 'It has been employed to a considerable extent in the preparation of delineations of *Jupiter* and *Mars*. The former of these planets has exhibited in the last year some appearances never before recorded; and it has appeared very desirable to register as soon as possible anything which seems to indicate a change in the constitution of that great body."

Mr. Carpenter during that year made a series of most careful drawings, showing all the flocculent port-holes and reddish colour of the equatorial region, bright eggs, and elliptic markings, which have been so noticeable during the last two years.

The next minimum of Sun-spots occurred early in 1866.

I have been able to find very few drawings of *Jupiter* in or near to this year, and my conclusion is, that there must have been but few conspicuous bright or flocculent markings to attract attention.

The Rev. T. W. Webb has been so good as to examine his

observatory note-books for drawings of *Jupiter* from 1863 to 1867. He only finds two drawings during this period with anything like white egg markings; the first on June 23, 1863, and the second in August 1866, with two white spots; one in a southern, the other in a northern belt; but he thinks that they have evidently nothing to do with the egg markings of 1860–1870.

It is unnecessary here to give any account of the reappearance of the same phenomena during the recent Sun-spot maximum. I append a Sun-spot curve for the period 1834–1870.

On account of the interest attached to the question of the variability of the Nebula of η *Argûs*, the Council have determined to print as well Mr. Abbott's communications as the remarks upon them by the late Sir J. F. W. Herschel, and the Astronomer Royal. For convenience of reference it may be mentioned that Mr. Abbott's former papers are printed in the *Monthly Notices*, vol. xxi. p. 230 (June 1861); vol. xxiv. p. 2 (November 1863), with plate; vol. xxv. p. 192 (April 1865, paper dated 18th February); and vol. xxviii. p. 200 (May 1868, paper dated 29th February), with a plate; and that there is a paper by Sir J. F. W. Herschel, vol. xxviii. p. 225 (June 1868); and one by him and Lieut. Herschel, vol. xxix. p. 82 (January 1869), with five plates.—Ed.

Some further Observations on the Variable Star η Argûs, and its Surrounding Nebula. By F. Abbott, Esq.

I now forward to the Society, after a period of two years, a third drawing,* with some remarks in reference to more recent observations on the inequality of motion, and variation in aspect of η *Argûs*, and the surrounding Nebula.

On my last communication some critical remarks were made, with a view of disproving that which I never intended to prove. This, in all probability, arose from my not having expressed myself with sufficient clearness in my remarks on the drawing which accompanied the observations. In this way the road to truth often runs through the midst of error, but that does not in any way alter the fact that great changes have been, and still are, taking place in the object under consideration.

Perhaps the field given in the drawing of 1868 may be somewhat too large, as it was my first intention to have made it larger; but, finding that the changes were principally confined to η and the so-called Lemniscate,† I confined the drawing to the size of the field given with the eye-piece.

* See plate of η *Argûs* as taken Jan. 28, 1870.—Ed.

† I scarcely think this term a good one :—Lemniscate, or Lemniscus; a curve formed as the figure 8, or a bow tied of a riband—Barlow and B. H. Smart. Such a curve is closed in the centre, which is not the case in the Cape drawing, it being there shown as a long enclosure slightly compressed in the centre. It was in this compressed part that the star η appeared when out of the dense Nebula.

As some objections have been raised on the ground that larger optical means than those employed by me were required for recording truthfully the changes which take place in this object, it may be well, perhaps, to state that the 5-feet Equatoreal previously mentioned is not the only instrument employed. Other telescopes have been used in the open air, from a 3½-feet Cook and Sons, to a 7-feet Dollond, with, for the whole, a complete battery of micrometers and eye-pieces, giving magnifying powers of from 25 to 450. This statement may go towards proving that observations can be correctly made without very large instruments.

In Mr. Proctor's article on the Nebula in *Argo* (*Fraser's Magazine* for December 1868) it is stated not to be quite clear that the stars which appear in my drawing of 1868 have been really copied from the view given by the telescope, &c. In reply to this, I beg to remark that all the drawings, the present as well as the former ones, were carefully copied from the object, as described in the *Astronomical Register* for January 1869. There is little doubt but that Mr. Proctor's views on this subject would be much enlarged if he had the opportunity of seeing the star and Nebula as they appear in the telescope, when above the Pole, at Hobart Town.

By these means, and having seen the object in the Melbourne reflector, I feel armed with sufficient courage to communicate what I have observed regarding the changes which have taken place in this object during the last two years.

In comparing the present drawing with that of 1868, it will appear clear that alterations have taken place, both in the magnitude of the star η and in the dispersion of the Nebula; and from what follows it will be seen that the remarks made by Professor Loomis and others on the period of this star are premature. When in Melbourne in June last, and while observing the object, Mr. Ellery considered the star η to be of the seventh magnitude, and Mr. Le Sueur thought it 6½. On my return to Hobart Town, I continued to observe it, and from careful comparisons made with the stars given in the drawing, and recorded in the Cape Catalogue, it cannot be of more than the seventh magnitude. In the Cape Catalogue there are two stars of the sixth, and nine of the seventh magnitude; the remainder are all low magnitude stars. The two sixth magnitude stars are out of the field. With the nine seventh magnitude stars in the field, they are by careful comparison exactly the magnitude of η, which is left amongst them not marked.

The magnitudes of these stars are those given by Sir J. Herschel, and may be considered correct.

Measures are recorded to have been made of these stars by small transit means; but, from my own experience in transit observations, I believe it to be all but impossible to measure correctly such a cluster of small zenith stars by such means. Two years ago I dismounted a 24-inches transit by Varley, in order to replace it with a 30-inches by Dallmeyer, made with a deep dia-

gonal eye-piece, for the purpose of reaching small zenith stars up to the seventh magnitude, as agreed upon for correcting any error in longitude between Hobart Town and Melbourne. Mr. Ellery selected forty-nine such stars, which were to be used at both places, reversing the instruments at each observation so as to eliminate any errors. With these means, and for this purpose, I find it difficult, and only under very favourable circumstances possible, to reach stars of the seventh magnitude.

My reason for not attempting measures with the Equatoreal is, in consequence of a long-experienced difficulty arising from the want of clock movement, which I consider indispensable for the accurate measurement of distances. I preferred, therefore, an eye-and-hand drawing, when the object has been in a convenient position, approximately 75° from the meridian towards the east, and 35° from the zenith.

The alterations which have taken place in the Nebula since 1868 will at once be seen on an inspection of the drawings, and by comparing them with each other. In the Cape Monograph, the dark space is an inclosure. In 1863 it had two openings, one at each end. In 1868 there were four openings; and now, in 1870, there are five which expose a number of isolated and distinct stars, rendering it difficult, but from position, to know which is the star η *Argûs*. There is attendant on these changes an increase of light which is notable up to the present time. During the last three full Moons (December, January, and February), when both the object and the Moon are approaching the meridian, the light around η *Argûs* is distinctly seen when all other Nebulæ are shut out. This increase of light may arise from one of two causes; either the Nebula has become concreted into isolated stars, or in its dispersion it has laid bare distinct stars which give out more light. The same physical forces that have worked out our own solar system are still at work in the stellar universe. Creation is still going on, and why not?

Private Observatory, Hobart Town,
Tasmania, Feb. 15th, 1870.

P.S.—I may mention, in conclusion, that I have on several fine nights tried one of Mr. Huggins' hand spectrum telescopes, as described in the *Proceedings of the Royal Society*, vol. xvi. No. 98, without being able to detect any appearance of bright lines in the Nebula of η *Argûs*, such as those seen in that of *Orion*.

Remarks on Mr. Abbott's foregoing Paper on η Argûs. By Sir J. F. W. Herschel, Bart.

Pursuant to the request of the Council of the Royal Astronomical Society, I have carefully perused Mr. Abbott's communication on η *Argûs*, dated Feb. 15, 1870, and examined the dia-

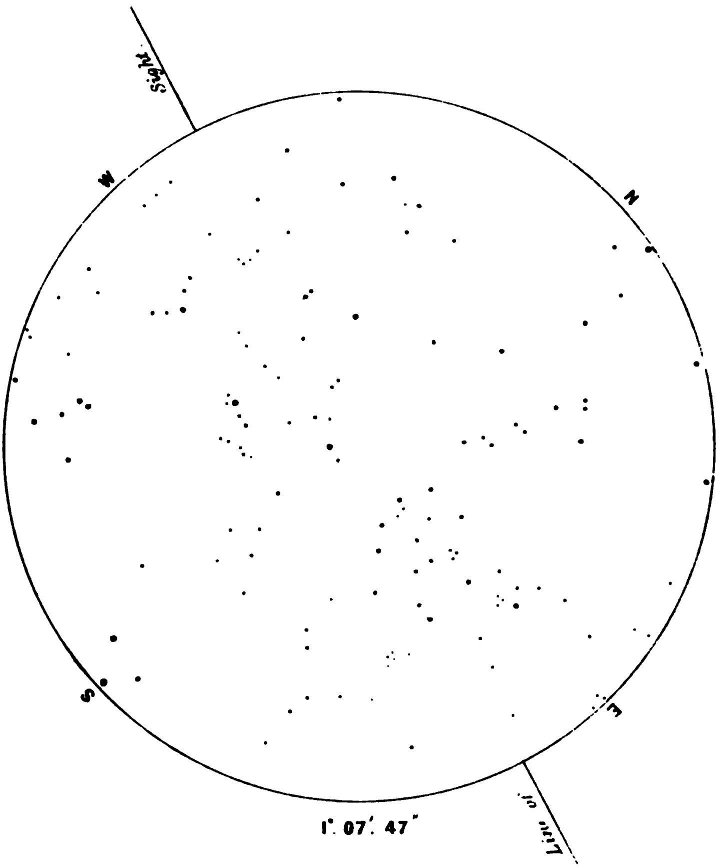
η ARGUS AS SITUATED TO THE DARK SPACE.

VARIABILITY OF THE SURROUNDING NEBULÆ,

WITH THE MAGNITUDE AND POSITION OF ACCOMPANING STARS.

UP TO THE PRESENT TIME.

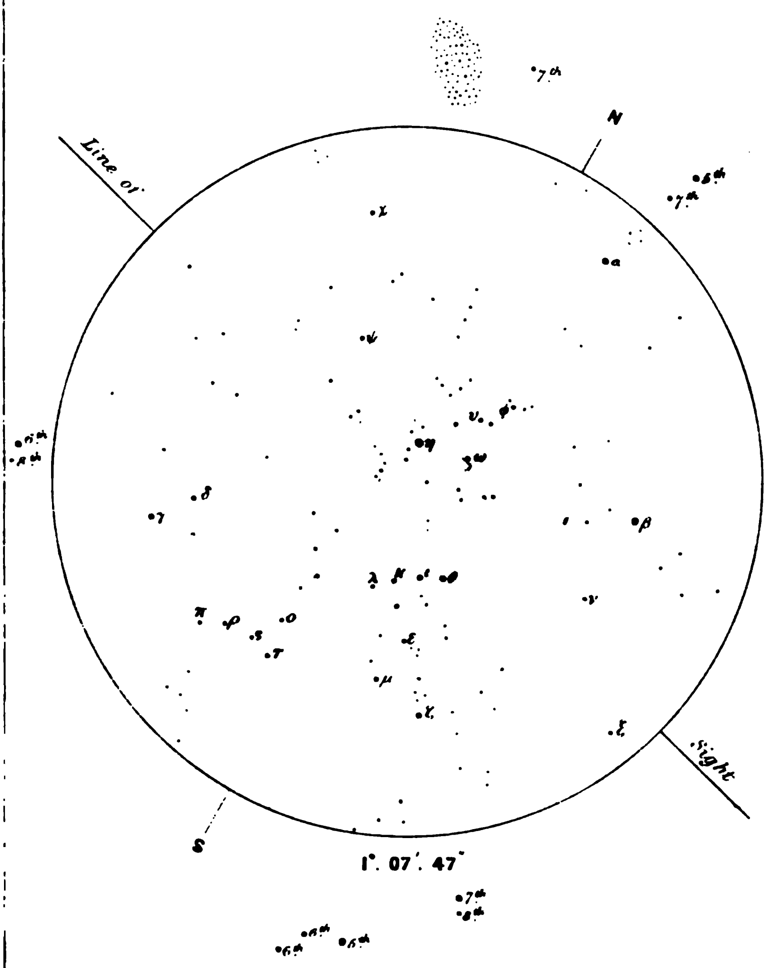
1870.



TAKEN JANUARY 28TH 1870.

Litho Cambridge.

APPROXIMATE DISTANCES AND MAGNITUDES OF THE PRINCIPAL STARS AND NEBULÆ SURROUNDING η ARGUS. TAKEN AT HOBART TOWN, FEBRUARY 1871.



gram accompanying it. Comparing the latter with Mr. Abbott's former representations of the Nebula, viz., that for 1863, in vol. xxiv. p. 5 (*Monthly Notices*), and that for 1868, in vol. xxviii. p. 200, the amount of variation is so astonishing that, considering all three to have been figured by the same observer, using (at least for the two last) the same telescope (a 5-feet Equatoreal, aperture not stated), it would seem to be doing injustice to Mr. Abbott not to place this last drawing also on record in the same manner as the others.

At the same time there is this difficulty. On comparing the present diagram with that in vol. xxviii. p. 200, it will be seen that, as regards most of the principal *stars* laid down, and several of the smaller groups, they exhibit on the whole a sufficient general agreement, considered as eye-drafts, while both are utterly irreconcilable both with my son's configurations (*Monthly Notices*, vol. xxix. p. 85), and with my own recorded observations, with which the latter entirely agree. On carefully examining the *present* diagram, I find it open to the very same remarks which I had occasion to make in my former communication on the same subject, viz., that there is not one among all the stars delineated which I can identify with any of those laid down in my own drawings and catalogued positions. The orientation (which I had surmised might possibly have been mistaken by Mr. Abbott in copying, or by myself), being here placed, by its reiteration, beyond a doubt; the most superficial inspection suffices to show that there is no correspondence whatever between us; and that his field of view of 1° 8′ diameter differs as completely from a similar field, in my monograph having η *Argûs* near the centre, as if the telescope had been directed to quite a different part of the heavens. For example, Mr. Abbott places within a distance of 11½′ (on the scale of his drawing) from η, five stars of magnitudes at least equal to η, that is, 7 mag. (if I am right in supposing the large star a little south-preceding the centre of his figure to be intended for η, and according to his former figure it can be no other), while in my monograph only one star of that magnitude (marked O) occurs within that distance; and five of the 8th magnitude, F, D, D′, C, K′, which however stand quite differently in relation to η from any of the larger ones in Mr. Abbott's diagram.

This would, however, be of little moment, were it permitted to suppose that attention had been given only to the delineation of the Nebula, and that the stars had been put down at random or with little regard to their real configurations. Mr. Abbott, however, in the paper which accompanies his diagram, distinctly repudiates this supposition, and insists on the correctness of his representations of the stars in the field of view delineated; not, indeed, as micrometrically accurate, but as careful eye-drafts. "All the drawings," he says, in reference to this especial point, "the present as well as the former ones, were carefully copied from the object."

Under these circumstances, were Mr. Abbott resident in England, I should feel disposed to recommend referring the communication back to him (*accompanied with these remarks*) for such elucidation of the causes of the discrepancy in question as the case may admit, and with a request that he would furnish *some* instrumental determination (however rough) of the differences of R.A. and P.D. between η and the eight (or nine?) other stars of equal magnitude with η, which he states to be included in his diagram, as also the *apertures* of the telescopes used; and, deferring the final decision of the Council as to the publication *in extenso* of the communication, to notice in the forthcoming Number of the *Monthly Notices* of proceedings, the fact of its having been read, with mention of his having observed on January 28, 1870, the Nebula to consist of five nebulous arcs or masses convex towards each other and towards η, which occupied a position at or near the point of embranchment of the same number of non-nebulous roads or channels running outwards in five different directions.

The Council, however, will best judge whether such a course is advisable, taking into consideration the length of time which must elapse before a reply could be received from Hobart Town.

Collingwood, May 19, 1870.

The substance of these remarks was communicated to Mr. Abbott in a letter dated 13th June, 1870, and he replied thereto in the following communication:—

In reply to the queries set forth in the letter of the 13th June last, I may state that it is not my intention or desire to dispute either Sir John's or Lieut. Herschel's configurations of the star η *Argûs* and its Nebula, but to draw the attention of the astronomical world to the altered features of both, with a view of ascertaining a solution of the changes seen in this most remarkable object.

My own opinion is, that no two widely different instruments will show the gradations of nebulous objects exactly alike; it will require a standard instrument. In my own case I have used the 5 feet with a 4-inch clear aperture, a comet eye-piece of 28, both perfect, for twelve years. And as a check, in the open air, I use a 3½-inch Cook and Sons, with a Kellner's orthoscopic eyepiece of 50.

Most of the questions put to me have been explained in former papers, excepting as regards measurement. In this I am not at all inclined to dispute either Sir John's or Lieut. Herschel's statements; what I contend for is the diminution in the size of the star η, and a continued, rapid breaking up and displacement of the Nebula surrounding it; both sufficiently apparent to any one acquainted with the object, and to be seen on fine nights with the naked eye.

The last question you require to be answered is, "Can I give any elucidation as to the difference?" &c. This borders on a physical question, and on that point I regret to have caused some displeasure to Sir John Herschel, by quoting a passage in the fifth edition of his *Outlines*, p. 639, sec. 871.

The object is now getting below the Pole, and daylight fast increasing; but when a favourable opportunity occurs, I will not omit to pay every possible attention to the requirements contained in your letter, feeling convinced, notwithstanding the contrary opinion, that the fluctuations which have taken place in *Argus* will some day become better known.

With regard to the fluctuations, distance and position very materially affect visible motion, and the changes about η are physical, if any, and therefore it would be necessary to adopt a real standard instrument, such as the old 18-inch erected at the Cape of Good Hope in its former position.

P.S.—I cannot refrain from mentioning my surprise at seeing an extract from a letter to the Astronomer Royal from Mr. H. A. Severn, of Melbourne, in the *Monthly Notices*, vol. xxx. p. 180. It is utterly at variance with the author's letters addressed to me, in all of which he not only acknowledges the changes in *Argus*, but also appears anxious that I should be credited with the discovery.

Surely there must be some mistake in the extract from the *Notices*. I have this moment received another letter from Mr. Severn, in which he expresses the same opinion on the fluctuations as before.

Private Observatory, Hobart Town,
Tasmania, 7th September, 1870.

There has since been received from him the following further communication:—

On η Argûs and its Surrounding Nebula. By F. Abbott, Esq.

To the Honorary Secretaries and Referees appointed by the Council of the Royal Astronomical Society to make inquiry as to certain alterations which have taken place in the star η *Argûs* and its surrounding Nebula.

In carefully looking over the drawings referred to, which were taken at Bangalore by Lieut. Herschel, with the object η *Argûs* 15° above the horizon, and also the *reversed* copy of Sir J. Herschel's, and on consideration of the discussion given with the drawings, I do not think that Lieut. Herschel's observations tend to disprove any one of the alterations which I have previously communicated to the Society, but, on the contrary, rather to confirm them. This, I think, will appear clear by an inspection of

the present drawing* and the answers given to questions put by the referees.'

The present drawing is taken with the same instrument as the former ones, the object in the same position approximately 80° above the horizon. The measures have been made with a *bar micrometer* by Cook and Sons, the bars being carefully traced in pencil on the drawing paper in such a way as to exactly fill the field of the telescope. All the stars visible were then dotted down, the distances from η of the sixth, seventh, and eighth magnitude stars were lettered, measured, and catalogued from a scale of equal parts; after which the micrometer pencil lines were rubbed out, and the Nebula inserted.

The first question put by the referees relates to a comparison of the positions of the principal stars and smaller groups, as shown in my two drawings, which are said to have a sufficient general agreement with each other, considered as eye-drafts, while they are irreconcilable with both Sir John and Lieut. Herschel's configurations. A simple inspection of my drawing of 1870, with the reversed drawing (A A), plate 4, of Sir J. Herschel, in the *Monthly Notices*, will show that the following principal stars hold a relative position, considered as eye-drafts, but not with the Cape monograph, as expressed in the letter D′, D—C′, C—(ν), κ—B—ζ—(ι)—522—558—640—337—383—415—(γ)—(λ), &c. &c. There are many other stars in my copy of 1870 that are not laid down in plate 4, pricked off from Lieut. Herschel's drawing.

The other question of note refers to my "having placed within 11½′ (on the scale of my drawing of η) five stars of magnitude at least equal to η, that is the 7th mag., while in Sir J. Herschel's monograph only one star of that magnitude (marked C) occurs within that distance, and five of the eighth mag.," and continues, "Can you give any elucidation of the cause of the discrepancy? also, if you would furnish some instrumental determination of the difference of R.A. and P.D. between η and other stars of equal magnitude."

In my acknowledgment of this letter to Mr. Wm. Huggins,

* Plate of η *Argûs*, as taken February 1871.—Ed. The drawing has upon it a reference to the several stars, as follows:—

	Mag.	Dist. from η. ′ ″		Mag.	Dist. from η. ′ ″		Mag.	Dist. from η. ′ ″
α	5	29 30	ι	7½	15 45	ρ	8½	30 20
β	6	26 0	κ	7½	16 10	σ	8½	29 15
γ	6½	32 0	λ	7½	17 30	τ	8½	29 40
δ	6½	26 30	μ	8	21 25	υ	8½	8 20
ε	7	22 30	ν	8	25 50	φ	8	12 0
ζ	7	31 20	ξ	8	40 0	χ	8½	26 40
η	7	—	ο	8	25 30	ψ	8½	13 50
θ	7½	16 15	π	8½	32 25	ω	8½	6 0

F.R.S., &c., I mentioned that it was not my intention or desire to dispute either Sir John's or Lieut. Herschel's configurations, but to call the attention of the astronomical world to the altered features of both the star and the nebula, with a view of obtaining a solution of the changes seen in this most remarkable object. I further stated that the above question was of a physical nature, and could only be answered as such.

On reference to my former papers it will be seen that mention is made, more than once, of the fact that the increase of stars of the same magnitude as η renders it difficult to know that star from others only by its position and a marked difference in the *light*. The present drawing will show a still greater and more remarkable number of stars of a similar magnitude.

It is to this cause I have so frequently referred, the increase of light, which I think is now clearly confirmed by both Sir John and Lieut. Herschel. At one of the monthly meetings of the Society Sir John considered the increased light in the object, as recorded, very strange, and remarked, "When I was at the Cape the Nebula could not be seen at all with the naked eye." Lieut. Herschel, at Bangalore, compared the light when the object was 15° above the horizon to *Pleiades* in *Taurus*.

Mr. Le Sueur, in his report on the Melbourne Reflector, says, "that the Nebula around η *Argûs* has changed largely in shape since Sir J. Herschel was at the Cape. The star shines with the light of burning hydrogen," and in his opinion "has consumed the Nebula."

The Catalogue will show that there are now in the same field two stars of the 6th, two 6½, three 7th, four 7½, four 8th, and nine of the 8½ magnitude, and it is literally crowded with others of from the 8½ to the 12th magnitude. Those lying outside the field and occupying an area of about 1½° have their magnitudes attached. The small cluster I take to be Sir J. Herschel's 3276, described as a "fine, bright, rich, not very large cluster;" if so, it is now a beautiful cluster of richly coloured stars, quite equal to χ *Crucis*.

It is almost impossible to define the boundary of the Nebula as it appears to be gradually fading away, and is not so distinct in outline as formerly.

The finest nights have always been selected for observing, and no delineation of the object has ever been given but what was an accurate representation of its appearance through the telescope.

Private Observatory, Hobart Town,
Tasmania, 17th February, 1871.

The foregoing Papers were referred to Mr. Airy, who remarks upon them as follows: —

I duly received the packet of papers relating to Mr. Abbott's observations on η *Argûs;* and with these I have perused

also the preceding papers in various volumes of the *Monthly Notices*. The subject is really a very puzzling one.

1. As regards stars only, the map of 1870 and the map of 1871* have so much difference (not a great deal) that I conceive them to be certainly independent; and yet they have so much similarity as to give strong probability to their faithful representation of the visible objects. See in particular the line of four stars convex towards η *Argûs*.

2. These four stars have some agreement, not quite good, with four of Sir J. Herschel's. But other stars in the concavity of the bend are wanting in Sir J. Herschel's.

3. When we look more closely to fundamental points, all is confusion. Mr. Abbott's observations were all made with a refracting telescope: in that case the order of the four cardinal directions on the map would be $\mathrm{E}\,{}^{\mathrm{N}}_{\mathrm{S}}\,\mathrm{W}$ (turned round, in any degree). But in the 1870 map he has marked them $\mathrm{W}\,{}^{\mathrm{N}}_{\mathrm{S}}\,\mathrm{E}$: in the 1871 map he has marked only ${}^{\mathrm{N}}_{\mathrm{S}}$. The ${}^{\mathrm{N}}_{\mathrm{S}}$ in the two maps differ about 45°.

4. In each of these maps is a line (different for the two) which he calls "Line of Sight." What this means I have not the slightest idea.

5. In points of geometry, therefore, Mr. Abbott is a most inaccurate man.

6. It is impossible for us to publish maps in this state.

7. As regards the delineation of the nebula, I cannot make out anything. The drawing in the second map, dated February 1871, agrees EXACTLY with Lieut. Herschel's drawing, 1868, Nov. 23, published in the *Monthly Notices*, 1869, Jan. 8 [misprinted 1868 in the *Monthly Notice*]. This drawing (Lieut. Herschel's) had undoubtedly reached Australia. Has Mr. Abbott copied it? Mr. Abbott's first map, dated 1870, Jan. 28, is irreconcilable with Lieut. Herschel's, which preceded, or with his own which followed.

8. While, therefore, I do concede to Mr. Abbott the merit of first pointing out that the nebula has shifted its position with regard to the star η *Argûs*, and has changed its form materially (both which points I regard as certain), I feel most strongly that his maps are utterly unfit for publication.

Allow me to suggest that papers of this kind ought to be preserved more carefully. Mr. Abbott's maps were crimped up till I could not read them. I have flattened them, and will endeavour to persuade Mr. Dunkin to keep them flat.

Royal Observatory, Greenwich,
1871, May 25.

* See the two Plates of η *Argûs*.—ED.

On the Nebula of η Argûs.

(*Letter from Lieut. Herschel to J. F. W. Herschel, Bart.*)

I hope I have not wasted time in examining Mr. Abbott's sketch of Nebula *Argûs*. It is only by accident that I see the *Monthly Notices of the R.A.S.*, so when at the Madras Observatory lately I took a tracing which I have here pricked off to send you, because I find that the stars marked are not so unidentifiable as I supposed. I have lettered a few; the others will, I dare say, be recognised as soon as the clue is given. On the whole, they appear to be not very incorrectly placed.

It is evident, I think, that in making this drawing a low power was used, which failed to show such details as the lemniscate. The broad outlines are not very dissimilar from my low-power sketch (No. 1, *Monthly Notices*, Jan. 8, 1869).

Mr. Abbott appears to have got wrong in his N. and S. points, if I have traced correctly. Is it rash to suspect that he has also mistaken η? In a letter to the editor of the *Astron. Register* (V. No. 73), he speaks of it "leaving the thick part and fixing itself in the dark space." It is not so in his own drawing, and it certainly is not so in reality, if my drawings are worth anything at all. Neither does he appear to have so much as recognised the lemniscate. He also speaks of "a dispersion of the stars," but his own drawing, as I now show, places most of his stars in approximately their right relative places. Surely all this betokens *non-recognition* on his part of the object he was examining, due probably to an inferior magnifying power.

If his chart of the stars is as correct as I think (I have no copy but this hasty trace at hand, and cannot find your engravings, unfortunately) every atom of evidence of change in the *nebula* which *he* adduces is swept away, because he at the same time testifies to a "scattering of the stars," which his own drawing proves to be a delusion.

As for the non-interpretation of his chart, the truth seems to be that a low-power drawing has been looked at as if it were a small portion full of detail. You speak (in your letter to the late Admiral Manners) of the "breaking up of the well-defined southern border by two clear open channels, which forms so marked a feature in Mr. Abbott's delineation." I think you will now see that the latter is on too small a scale to show the lemniscate, at any rate that there is no trace of it, and that the "channels" are distinct and real features in another part of the nebula.

If this mystery is now cleared up, it will be well to hesitate in accepting the apparent changes shown in *my* drawings as well. They will probably be shown, under higher power and increased light concentration, to be likewise mistaken attempts to show details insufficiently clear.

Bangalore, May 7th, 1871.

A Proposal for a series of Systematic Surveys of the Star-Depths. By R. A. Proctor, B.A. (Cambridge).

When we consider the vast addition to our knowledge, and the yet vaster widening of our conceptions respecting the star-depths, which resulted from the labours of Sir William Herschel and the great man whose death science is now deploring, we cannot doubt that more complete surveys, if such could be carried out, would well repay the pains bestowed upon them. I apprehend that not the least among the purposes which the elder Herschel proposed to fulfil when he commenced that first great survey was to show astronomers how much the survey of the heavens was needed; and I imagine that he would have been the last to approve of that supineness (arising, perhaps, from an exaggerated respect for his labours) which has prevented all save his son from pursuing the path which he was the first to indicate. As to the opinion of Sir John Herschel on this matter, it is unnecessary that I should speak; because he has in many parts of his works urged in his own earnest manner how desirable it is that the celestial depths should be studied much more closely than they could be by only two observers, however skilful and energetic, or however patiently they continued their labours for many successive years.

I believe that what is now specially required is a series of systematic surveys, proceeding on a principle quite different from that on which the Herschels found it necessary to pursue *their* researches. A first survey was very properly, or rather, it was necessarily applied with reference rather to the average distribution of the stars than to the special laws of distribution which may be found to prevail either when we extend our survey over the different parts of the celestial sphere, or when we vary the range of our vision by employing different telescopic powers.

I venture now to impress most earnestly on those who have sufficient leisure and possess the necessary instrumental means for carrying out systematic observation, the extreme importance of surveys of the star-depths on methods devised with reference to both these relations.

Let me briefly indicate what is at present wanted, noting at the same time, that the proposed observations should only be regarded as first steps in a progressive series of researches directed to the solution of the noblest of all the problems astronomers can deal with,—the determination of the laws (so far as they are discoverable) according to which the heavens are constituted.

(1.) I have already mapped down isographically the stars visible to the unaided eye; and I think that no one can study my isographic chart, or the numerical statistics which accompany it in the second edition of my *Other Worlds*, without feeling that

even what the heavens disclose to the unaided eye has a significance which has too long been suffered to escape recognition.

(2.) I am engaged in mapping down isographically the stars in Argelander's charts of the northern heavens—324,000 stars in all, about 310,000 north of the equator, and about 14,000 within two degrees south of the equator. When this chart is completed, it will serve to show, so far as the northern heavens are concerned, what are the laws of distribution among those stars which lie within the range of a telescope 2½-inches in aperture. The correspondence between these laws in certain parts (at least) of the heavens, with those observed in the distribution of naked-eye stars on the one hand, or of the Milky Way, nebulæ, &c., on the other, will throw light on many questions of interest,—for instance, on the numerical relations of large and small stars (stars really large and small, that is), and (consequently) on the evidence as to the distances of stars of different apparent orders. Light will also be thrown on many other questions of importance. That this is so I shall be able (I already know from the progress of the work) to establish; but at present it will suffice to notice, as evidence of the importance of such researches, that Struve, taking only a small portion of Argelander's charts, and dealing with that portion only by averages,* not only as respects extension in right ascension and declination, but also as respects probable extension in space, was yet led to results of extreme interest if admitted, and highly suggestive even if regarded as still open to question.

The system of charting with reference to an aperture of 2½-in. requires extension over the whole southern hemisphere. The results of this extension alone would be (I venture to predict) of the utmost possible interest. For the purpose of increasing our knowledge of the constitution of the star-depths, actual charting would not be needed, but only such a process of statistical enumeration as I propose in the case of larger apertures.

(3.) The Council of this Society have kindly placed at my disposal the Sheepshanks Equatoreal No. 3, having an aperture of 4½-in., and I propose to apply this instrument to the enumeration of stars lying within the increased range belonging to its aperture. The plan I propose to follow is to count the number of stars seen in equal fields taken all over each region surveyed, and then to map down the result isographically. It is probable that the very limited amount of leisure time I can devote to such researches may prevent me from surveying more than a small region of the northern heavens. But as my special object is to show what lessons such surveys are calculated to teach, I shall be well satisfied if I can thus, by example more effectively than by precept, engage others who have more leisure and instruments of

* How rough these were (intentionally, of course) is shown by the fact that the charted results did not even indicate the division of the Milky Way into two parts over a region crossed by Struve's section. "C'est une suite naturelle," says Struve on this point, "de ce que notre recherche du disque ne s'est point faite dans tous les détails, mais *par heures entières* d'ascension droite."

about the size named to extend this survey to much larger regions. If the results are not charted by the observer himself he should keep a gauge-book from which an isographic chart could be formed hereafter.

The southern heavens also should be surveyed with a telescope of about 4½-in. in aperture.

(4.) A survey on a similar plan should be carried out over the whole heavens with a telescope 9-in. in aperture.*

(5.) Star-gauging with powers about equal to those of the telescopes with which the Herschels surveyed the heavens, should be carried out on a much more complete plan than was possible in the case of astronomers engaged like the Herschels on many different branches of research simultaneously. In the present position of sidereal astronomy, more is to be gained by the complete survey even of a small region in this way (followed by careful isographical charting) than by an incomplete survey depending on a law of averages which has been proved *not* to prevail in stellar distribution.

(6.) A good 4-ft. speculum should be applied to the same purpose, even though half-a-dozen generations might have to be occupied in the work.

Feeling absolutely certain of the extreme interest and importance of the results which would follow from such surveys as I have here advocated, I am by no means deterred by the largeness of the labours involved, even in the complete survey of the heavens with small apertures, from urging those who can do so to join actively in a work so valuable. There are so many amateur astronomers who have fine telescopes for which they can find no employment, and so many others who *do* find employment for their telescopes, but after a fashion tending in no sort to advance our knowledge, that the work ought not to languish for want of recruits. And I should imagine that no training, even, would be wanted by a large number of these recruits; because the eagerness with which telescopists are trying to divide difficult double stars, or to see planetary features which their telescopes are just *not* able to show, and so on, seems indicative of an earnest desire to acquire a fitness for useful work. I have named certain apertures which should at once be applied to the survey, but reflectors and refractors of 5, 6, 7, or any number of inches in aperture, could be most usefully employed in sidereal survey.

* I speak here with reference to refractors. If reflectors are used in such a survey a careful comparison of their light-gathering power, and that of refractors employed in the same sort of work should be instituted beforehand. If this power were brought to an equality, or nearly so, different parts of the heavens might be simultaneously surveyed by observers using reflectors or refractors.

On a Contrivance by which it is designed to measure Time automatically, in taking Star or other Transits. By Capt. J. Herschel, R.E.

There are two ways of effecting the object of this paper. The one is to describe all the parts of the proposed mechanism, leaving it to be inferred, or subsequently explained, in what manner they answer their several purposes, and conjointly secure the end in view; the other is to start with the object to be attained, and show how the requisite conditions may be met. The last will be here followed, as offering a better chance of perfecting the contrivance, as a whole, through the substitution of other mechanical artifices than those which present themselves as at first sight sufficient.

In observing a transit, as hitherto practised, the estimation of Time is an essential element, where by "estimation" is to be understood the combined operation of the senses and the intellect. The instant of an occurrence has to be estimated, and instruction transmitted to a recorder, automatic or other, to note the same. This complex series of duties is liable to miscarriages, from which the estimation of position is free. Is it not possible to obviate this? Cannot the passage of the star's image across the wire produce the same effect directly, as that which is now produced indirectly through the medium of the observer's senses, and exertion of nervous energy? Or if this is not yet attainable, cannot the observer be enabled to throw so much of himself into the work as to cause the arrival of something *material*, coincidently with the immaterial image; and *thereby* put in action at the moment of arrival the train of forces which mark the instant? This is practicable. Let a diaphragm with a single vertical wire be driven across the field of view at the same rate as the star, and (as regards the wire) coincidently with it. In its journey let it produce the contact which completes the galvanic circuit, and impresses the time-record on the chronograph. If this event takes place at a fixed stage in the journey, it is evident that the time will always be recorded when the wire is in a fixed place, and, therefore, since the wire is to be supposed to accompany the star, when the star is in that same place. This is equivalent to determining the instant of transit over a fixed wire. And it is perfectly automatic, for the observer is not concerned with it at all, except in keeping the star and wire accurately together during their journey across the fixed point.

But how is he to do this? The only answer is, that mechanism must be provided which will enable him. By releasing a wound-up spring acting on a fan through a train of wheels, the wire diaphragm must be drawn across at any rate not exceeding that of an equatorial star. If so released when the entering star reaches the wire, the two will travel across together—not necessarily coincident, but in close proximity. Further adaptation

must enable him to restrain, or urge forward with the needful delicacy, the moving frame and wire, and so effect an intersection as though both were stationary. While one hand is attending to this, the other will hold the contact-key which is to allow or forbid the record being made, according as the observer is satisfied or the reverse with the intersection he has made, at the time of reaching the stage where contact will take place. Of course there may be any number of such stages.

The traversing mechanism is, no doubt, that which will require most ingenuity. The work which it has to do is exceedingly light, and therefore it may be made with delicacy, and with due regard to the danger of perceptible vibration imparted to the tube. Attention may also be well bestowed on that part of it which regulates its mean speed. If possible the setting of the telescope should act directly upon it, so that (within the limits of N.P.D. which experience may show to admit of its action) the speed will in every position correspond, as closely as ingenuity can make it, with that of a star towards which the tube is directed. There is ample room here for the skill of a machinist; but there does not appear to be any insuperable difficulty, since all that is really requisite is a nearly uniform motion, susceptible of moderate intentional and subsidiary acceleration, or retardation (such as may be produced by a spring acting on the circumference of a plain wheel), and subject to gross regulation by previous adjustment. An arrangement will also be necessary by which the action of the machinery on the wire frame can be thrown out of gear, so that the latter may be quickly moved to any part of the field, to pick up a star that may have entered unawares. These difficulties are not underrated; but the very great importance to astronomy of any practical means of relieving the observer from the duty of estimating time minutely seems to make them worthy of being surmounted.

There does not seem to be any corresponding difficulty in arranging the parts which are to serve the essential purpose of recording the instant of transit of the *wire*—as distinct from that of the star which it represents. An isolated needle-point projecting from the sliding-frame, and isolated hair-springs projecting from its guide frame, seem to meet all the requirements. It is, however, conceivable that the personal equation, so to speak, of the battery may vary, owing to its greater strength at one time than at another. But it will certainly be constant for the whole of each transit, and will probably be under complete control eventually. As it would be highly desirable, indeed essential for transits below the pole, or with reversed pivots, that the mechanism should permit of the wire-frame being traversed in either direction, backwards or forwards, in order that the wire may likewise accompany a retrograde object, the needle-point and springs must come into action against each other indifferently on either side. This also will cause a "personal equation" ... it may be hoped, will not be difficult of determination.

Whether the above conception is or is not susceptible of practical realization, it is difficult to believe that the method could not be made to secure, in some such way, the desired result of a mechanical determination of time. If it can, it must prove of great value to an observer of transits, by relieving him of the duty of estimating *the time* of an event.

Bangalore, September 8th, 1870.

Prof. A. S. Herschel, in a letter dated Collingwood, June 14th, 1871, addressed to Mr. Lassell, and communicating to him the foregoing paper, writes:—

In the autumn of last year my brother also sent me a paper on a contrivance for automatic registry of the Times of Transit of a star, across the wires of a transit telescope. Until the paper had been submitted to some good practical mechanical and astronomical authority, my brother did not wish it to be communicated to the Astronomical Society. I then sent the enclosed copy of it to my late father, with the small drawings 1 and 2 (following it) of the Eye-end of a Transit-telescope, showing a contrivance of a revolving sphere, driven by clockwork and driving (by rubbing) the head of a micrometer-screw. By inclining the axis of the sphere at different angles to the axis of the screw, different rates of revolution of the screw might be obtained, corresponding very nearly to the different polar distances at which star transits would be observed.

On a second page a plan of a pair of "*Marlborough wheels*" is shown, with a moveable pinion engaging them both, and capable of being made to travel round them in either direction. When fixed, it transmits the power of the clock to the telescope (or to the micrometer-screw at the eye-piece), with a fixed velocity-ratio, depending on the proportion of the number of teeth in the two Marlborough wheels. When it is moved by the hand, in one direction, it increases, and in the other direction it diminishes, the velocity ratio of the Marlborough train; and it thus enables an observer to wait for, or to overtake, and thus to bisect a star.

The objection urged by my late father against any such construction for automatically registering the *Times* of transits, was, *that it burdens the eye-piece of a telescope* with cumbersome machinery, which is the part of the telescope that requires to be most light, and free from tremors. I also showed my brother's paper to Professor Grant, at Glasgow, who recommended its communication to the Astronomical Society. But before presenting it to you, I hoped to be able to obtain Mr. Airy's opinion of its merits, which my very close and constant occupation at Glasgow, during the last Winter Session, unfortunately prevented me from doing. I need scarcely add that my attempt to supplement the paper with a few mechanical contrivances is of no value or importance, compared to the *practical scientific interest* which attaches to my brother's original suggestions.

The drawings and explanations referred to in the letter were as follow:—

a is a bevel wheel driven with constant speed by a spindle *p* which comes down the tube of the telescope from a clock fixed upon one of the piers or pillars of the horizontal axis of the telescope. A long brass arm A turns on the axis of the wheel *a* as a centre, and carries another large bevel wheel *b* at its other end, which is driven from *a* by the intermediate pair of mitre-wheels

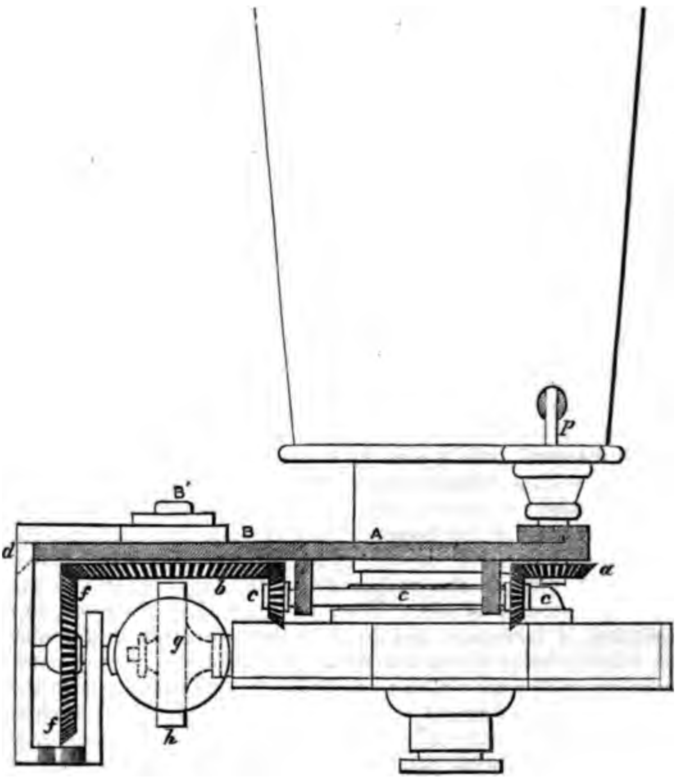


Fig. 1. View from above the Tube.

(*c c c*) on a common axis *c*, supported by the bar A. The latter ends at B in a circular plate B (a part of which is shown cut away at C, fig. 2, for lightness where it is not required); and B is pierced through its centre by the pin B′, which carries the bevel-wheel *b*, and also a brass plate, or radius, D, three times bent so as to carry the axis of another mitre-wheel, *f f*, which works in the teeth of *b*. This last axis carries the brass sphere *g*, which thus revolves with a constant speed, being wrought directly through the train of wheels, from the first clock-wheel *a*.

The sphere, *g*, rests with its whole weight, and with the weight of the lever A, and of its appendages upon the graduated head, *h*, of the micrometer, which is to be covered with an india-rubber band, and it will then be driven by the friction of the sphere. By turning the bent arm, D, round the pin B′, so as to give the axis of the sphere any degree of inclination to the vertical line, *g h*, fig. 2, corresponding to the N.P.D. of the star to be ob-

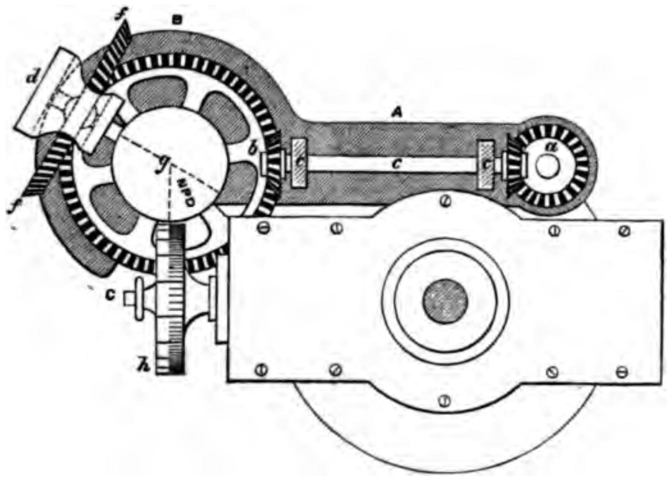


Fig. 2. Front, end view of the Tube.

served, the speed with which the sphere will drive the micrometer-screw head round, will be proportional to the speed with which the star will cross the field of view at that distance from the pole; and to facilitate placing the axis of the sphere at its proper inclination, the edge of the circular plate, B, can be graduated, and a vernier-aperture at *d* may be cut through the bent arm, D, so as to show the N.P. Distances to the nearest degree, or 15′, for which the sphere is to be set. A little adjustment by a fine tangent-screw would bring the speed of the wire-frame into exact coincidence with that of the star after it had entered the field of view, by a few trials. And the action of a smooth sphere upon a stout band of india-rubber would probably be very regular and uniform; or might, at least, be worth a practical trial.

Instead of the weight of the lever, A, and of its appurtenances pressing upon the head of the micrometer-screw, this part of the apparatus might be made as light as possible, and might be made to press uniformly against the head of the micrometer by means of an elastic spring.

A pair of Marlborough wheels (like those fitted by Messrs. Cooke to the clock-motion of Mr. Newall's large Equatoreal

telescope at Gateshead) placed in some part of the train of wheels between the clock and the telescope (see fig. 3), would enable an observer by turning a milled head, and thus making a pinion revolve backwards or forwards round the pair of Marlborough wheels, to retard or advance the motion of the sphere, g, and of

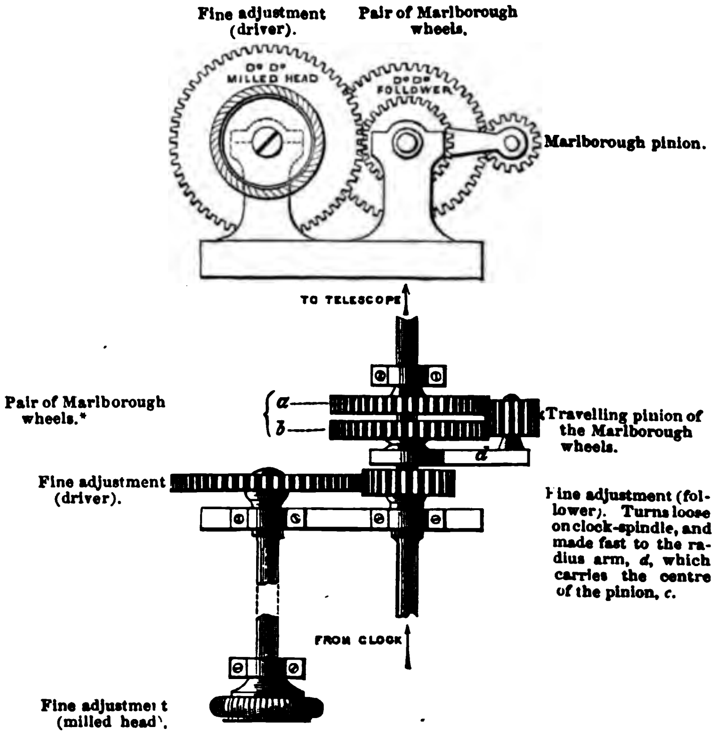


Fig. 3. Pair of Cooke's Marlborough wheels for advancing or retarding the motion of a telescope driven by clockwork, so as to give small adjustments in R.A.

the micrometer-screw which it drives in any manner; so as to make the micrometer-wire bisect the star. When the moveable pinion is left at rest, the motion of the clock is transmitted uniformly through the rubbing-sphere, g, to the wire frame of the micrometer, with the proportion of its speed exactly such as to

* The wheel a has a few more teeth than the wheel b, and moves slower than the wheel b, when the centre of the pinion c is fixed. When the pinion c is carried forwards round the two wheels a, b, the wheel a is also carried forwards, or it is made to advance. When c is turned backwards round the wheels a and b, the wheel a is also turned backwards, or it is made to return. The wheel a has the same diameter, and is on the same line of centres with b; and it has *so few more* teeth than b, that the difference of their pitch does not prevent both of the wheels from working equally well in the pinion c.

keep the star bisected by the wire as long as they remain together in the field of view. The frictional bearing of the sphere upon the screw-head will prevent the clock from over-winding and injuring the thread of the micrometer-screw if it should be inadvertently left in gear with it until the screw reaches the end of its range.

The Astronomer Royal remarks on the foregoing :—

Capt. Herschel's abstract idea, that a wire should be made by mechanism to accompany the star-image, and at a definite point should make a galvanic contact, is excellent. It implies the necessity of placing the wire on the star beforehand, and of giving the wire frame a motion agreeing *very accurately* with the motion of the star-image. And here is the practical difficulty.

Professor Herschel's mechanical plan for doing this is ingenious. But it implies, 1st, an original uniform motion (not easy to get); 2nd, motion through three engrenages of bevelled wheels (which would have abundance of shakes); 3rd, adjustible motion by the surface of a sphere rubbing a drum-head at different declinations on the sphere. (I doubt whether this can be made nearly correct enough, but it is very pretty.)

I do not think that the problem is by any means solved.

Note on the Curve traversed by base-end (remotest from fixed pivot) of the last prism of a Single or Double Automatic Spectroscope. By Richard A. Proctor, B.A. (Cambridge).

Writing down somewhat in haste the equations in the Note on page 206, I failed to notice that the polar equations to the curves traversed by the points P and Q (fig. 2, p. 207) can be very readily obtained. Thus let

$$AP = r \quad \text{and} \quad \angle PAO = \theta.$$

Then θ is the component of $\angle$ DOA; that is

$$\theta = \frac{\pi}{2} - 6 \sin^{-1}\left(\frac{a}{R}\right)$$

or

$$\sin\left(\frac{\pi}{12} - \frac{\theta}{6}\right) = \frac{a}{R} \qquad (1)$$

But

$$r = 2 R \cos\theta;$$

hence (1) becomes

$$r \sin\left(\frac{\pi}{12} - \frac{\theta}{6}\right) = 2a \cos\theta.$$

This is the polar equation to the locus of P. It may be written

$$r\left[(\sqrt{3} - 1)\cos\frac{\theta}{6} - (\sqrt{3} + 1)\sin\frac{\theta}{6}\right] = 4\sqrt{2}\,.\cos\theta.$$

The polar equation to the locus of Q is obviously

$$r \sin\left[\frac{\pi}{12} - \frac{1}{6}\left(\theta - \frac{\pi}{4}\right)\right] = 2\sqrt{2}\, a \cos\left(\theta - \frac{\pi}{4}\right);$$

or

$$r \sin\left(\frac{\pi}{8} - \frac{\theta}{6}\right) = 2\sqrt{2}\, a \cos\left(\theta - \frac{\pi}{4}\right).$$

It may be written

$$r\left[\sqrt{2 - \sqrt{2}}\cos\frac{\theta}{6} - \sqrt{2 + \sqrt{2}}\sin\frac{\theta}{6}\right] = 4a(\cos\theta - \sin\theta).$$

Occultation of 80 *Virginis by the Moon, observed at Forest Lodge, Maresfield, on May* 30, 1871. By Capt. Noble.

The star disappeared instantaneously at the Moon's dark limb

At $14^h\,25^m\,5^s$ L.S.T. = $9^h\,53^m\,13^s{\cdot}9$ L.M.T.

and reappeared at the bright limb (from behind a dome-shaped mountain)

At $15^h\,14^m\,48^s$ L.S.T. = $10^h\,42^m\,48^s{\cdot}8$ L.M.T.

These times are very good, although the atmosphere was unsteady. The power employed was 255, with my 4·2-inch Ross Equatoreal, and was adjusted on the star.

Observations of Winnecke's Comet. By the Rev. S. J. Perry.

Owing to the unusually bad weather in April and at the beginning of May, I was unable to observe this comet for a long time after its discovery. When the sky permitted of a first observation, the twilight and the nearness of the Sun rendered the object much less distinct than it had previously been, though it was still much more conspicuous than the comets of 1870.

The following positions may serve to complete the series of observations made by others more favoured by fair weather. The nearest Greenwich star *ι Aurigæ*, was chosen as the star of comparison, but an intermediate double-star was also made use of to diminish the chances of error.

	G.M.T.		R.A.	Decl.
	h m			
May 8	10 8	ι Aurigæ	4 48 34·2	+32 57 31·4
		Dist. of Comet	− 20 37	+ 2 5 30
		Comet	4 27 57·2	35 3 1·4

	G.M.T. h m		R.A.	Decl.
May 8	9 57	ι Aurigæ	4 48 34·2	32 57 31·3
		Dist. of Comet	−17 36	+1 22 35
		Comet	4 30 58·2	34 20 6·3
10	9 43	ι Aurigæ	4 48 34·2	32 57 31·3
		Dist. of Comet	−14 2.	+37 40
		Comet	4 34 12·2	33 35 11·3

Observations were also taken on the 11th and 12th, but the nearness of the comet to the horizon and the passing clouds rendered the results less trustworthy.

Stonyhurst College Observatory,
June 6, 1871.

On the Total Solar Eclipse of 1871, *December* 11–12, *on the Australian Continent.* By J. R. Hind, F.R.S.

The total eclipse of the Sun in December next would be most advantageously observed on the northern coast of Australia. I am not aware, however, that the track of country situated within the zone of totality is anywhere colonised, and to secure observations it would probably be necessary to send round observers from Sydney or Melbourne.

To afford an idea of the circumstances of the eclipse in this region, I have selected four points near the coast from the Admiralty map of North Australia, and have made special calculations for these points, taking the elements of the eclipse from the *Nautical Almanac.*

The positions selected are—

	Long.	Lat.
Vansittart Bay (S. extremity)	126° 18′ E.	14° 10′ S.
Mount Casuarina	127 37	14 23
Pearce Point	129 22	14 27
Groote Eylandt (S. E. extremity)	137 2	14 15
Cape Sidmouth	143 38	13 25

The following are my results:—

		Totality Begins. h m s	Totality Ends. h m s	Duration of Totality. m s	Sun's Alt. °
Vansittart Bay	Dec. 12	0 48 9·3	0 52 16·7	4 7·4	74
Mount Casuarina	,,	0 56 57·6	1 1 13·5	4 15·9	72
Pearce Point	,,	1 9 1·3	1 13 13·1	4 11·8	70
Groote Eylandt	,,	2 0 16·2	2 3 59·5	3 43·3	59
Cape Sidmouth	,,	2 41 37·2	2 45 3·9	3 26·7	49

The first and last contacts occur as below:—

		First Contact. h m		Last Contact. h m
Vansittart Bay	Dec. 11	23 15	Dec. 12	2 25
Mount Casuarina	"	23 24	"	2 34
Pearce Point	"	23 37	"	2 45
Groote Eylandt	Dec. 12	0 33	"	3 31
Cape Sidmouth	"	1 20	"	4 7

Local Mean Times throughout.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXI. *Supplementary Number.* No. 9.

Remarks on the Evidence brought forward on the question of a supposed Remarkable Change in the Great Nebula near η Argûs. By William Lassell, Esq.

On the diminution of brightness of η itself, the evidence is uniform and corroborative; showing that it has diminished from about 1·2 in 1837 (Herschel's notation) to about the 6th or even 7th magnitude, which latter it is now (1871) expressly stated to be by Mr. H. C. Russell, Government Astronomer, Sydney, New South Wales.

The first paper I find upon the variation of the Nebula is from Mr. F. Abbott, dated Hobart Town, June 1863, and is contained in the *Monthly Notices*, vol. xxiv. page 4. He therein says,

"The open space, as given in the Cape Monograph, is somewhat in the form of a dumb-bell, compressed in the centre and surrounded with nebulæ, in the most dense part of which is situated η *Argûs*. The appearance of the open space now assumes the form of a crooked billet, wide in the centre and open at both ends, with η *Argûs* situated within the open space, or dark part, and surrounded with an almost innumerable quantity of brilliant stars, many of which are arranged in groups, some being of a blue and some of a ruddy colour. They are remarkably brilliant in the dark space, and afford a good comparison with the variable star itself. . . . In 1838, when examined by Sir John Herschel, the star η *Argûs* was situated in the most dense part of the nebula, and was seen as a star of the first magnitude, and now in 1863 it is out of the nebula, and within the dark space it only appears a star of the sixth magnitude."

Then follows a rude diagram, in which no position with reference to east, west, north, or south, is indicated.

At page 5 Mr. Abbott says,

"That the star's right ascension has not varied so much

A

will be manifest. It is clear, then, that the dense portion of the nebula towards the east must have receded, leaving each end open, and η *Argûs*, together with about seventy stars ± up to the 14th magnitude, as seen within the dark space."

In a note is the following :—

"1863, May 23, a drawing made of the object —η *Argûs* quite distinct within the dark space, which is open at both ends."

Again Mr. Abbott states (*Monthly Notices*, vol. xxviii., page 200), in a paper dated Feb. 29th, 1868,

"The principal instrument used is a 5-feet Equatoreal by Dallmeyer . . . and a 5-feet portable refractor by Varley, . . . the eye-pieces in general use . . . are a Comet one of 28, an Orthoscopic of 45 and an Annular Micrometer of about the same power. . . . The beautiful soft white light given out by the nebulous matter about η *Argûs* appears to be produced either from the increased magnitude of the stars or the displacement of some of the nebulous mass, or probably from both, for, in the former case, it is difficult to say (only from its position) which is η *Argûs* and which is not, there being so great a similarity in its size and that of some of the accompanying stars."

In the Cape Monograph only about three stars so large as the 8th magnitude and about six of the 10th magnitude are so near η as a space equal to about half a minute of R.A.; the others are very much smaller, down to the 14th or 15th magnitude. If η, then, was not less than the 6th magnitude, it must have stood out conspicuously among all these; unless it be imagined that while η has been decreasing they have been increasing in something like an equivalent ratio. Again, the import of Mr. Abbott's final remark is scarcely apparent—"If, therefore, the line of sight is brought to correspond with the south of the Cape Monograph, the position of both drawings will be made approximately to agree at the time they were taken."

In another paper by Mr. Abbott (*Monthly Notices*, vol. xxv., page 192, dated Feb. 18, 1865), he says,

"In my former Notes I described the so-called dark space as resembling in shape a clearly defined 'crooked billet;' it now assumes a very different form, and, what is singular, Sir John Herschel describes κ *Crucis* in glowing colours—'like a rich piece of jewellery'—but he is silent as to the colours of the stars round η *Argûs*. In my former Notes I mentioned many of them as being of a ruddy colour, but now they are of decided colours—blue, green, and red, the two former predominating. If the telescope is turned from one object to the other it will be seen that, although Sir John Herschel has not overdrawn the beauty of κ *Crucis*, the object η *Argûs* is now much more superb, η standing out sharp and clear amidst a large field full of richly-coloured gems, with only a very small patch of nebulous matter seen under the telescope."

Mr. E. B. Powell, in a paper dated Madras, 9th March, 1864 (*Monthly Notices*, vol. xxiv., page 170), says,

"On the 15th April, 1860, I noted in my observation-book that the Nebula was 'much fainter than formerly,' while on the immediately preceding 23rd March the following entry occurs:— 'Nebula about η *Argûs* magnificent.' On the date first mentioned η is, in a rough sketch, placed out of the bright portion of the Nebula, and the lemniscate is described as a 'channel.' Several entries follow, noting both the openness of the lemniscate to the south and the greatly diminished brightness of the Nebula. The former fact was acquiesced in by Major Tennant, of the Bengal Engineers, whom I requested to examine the Nebula with the Lerebours Equatoreal of the Madras Observatory. On the 4th April, 1862, the following entry was made in my book:— 'η beautifully round and clean, out of the nebulous matter altogether; two patches of nebulous matter, with a passage between them, to the left, or preceding.'"

In a paper of some length, dated Collingwood, 8th June, 1868 (*Monthly Notices*, vol. xxviii., page 225), Sir John Herschel discusses, with abundant care and minuteness, Mr. Abbott's drawing (page 200), selecting several of the principal stars in it near to η, and comparing them, as far as possible, with the stars about η laid down in the Cape Monograph,—but there is really no sort of agreement between them.

On this Sir John remarks,

"What are we then to conclude? Not only the nebulous masses would appear to have drifted far away from their situations in 1835, but the stars of the whole region over an area of nearly two-thirds of a square degree, including stars of the 6th, 7th, and 8th magnitudes, to have also either assumed new configurations *inter se*, or to have bodily fled away and given place to a new set!"

In vol. xxix., page 82, is a paper by Lieut. (now Capt.) Herschel, dated Bangalore, Nov. 23, 1868, giving account of some observations, together with some drawings, of the Nebula made on the 22nd, 23rd, and 24th Nov. 1868. They are accompanied by some remarks of Sir John Herschel, from both of which I make the following extracts. The telescope used was "the Royal Society's 5-inch refractor, power 55 to 200." The drawings are "the appearances under powers of 55 and 125, copied from sketches made on the spot, the originals being retained." "Altitude at the time of observation about 15°, the air clear enough to have shown stars of the 3rd magnitude within a degree of the horizon, roughly speaking."

Of the stars inserted in sketch A, Sir John remarks that twenty of them are unmistakably identified with stars inserted in the Catalogue made by him at the Cape, of stars in the Nebula; and he further says,

"A decisive proof of the absence of any material shifting of position between the lemniscate and η *Argûs* is afforded by the small star [*v*] in a direction pointing from η to about half way between C and D. This star (No. 664 of my Catalogue) oc-

cupies, both in my engraving and in my son's drawing, precisely the same strongly-marked position, close to the very edge of the most characteristic projection of the Nebula, being just immersed in the Nebula in both."

The *Sydney Morning Herald*, of Saturday, May 13th, 1871, contains an account of a meeting of the Royal Society of New South Wales, at which a long paper was read by Mr. Russell on the Nebula of *η Argûs*. Mr. Russell states that, with the large Melbourne Reflector, after having been repolished by Mr. Le Seur,—"Mr. McGeorge has been able to see that *Eta* is still in the Nebula; and, in his drawing, which includes only the nebulous mass in which the lemniscate is situated, shows the very dense part near and north of *Eta*, the enclosed space, and some remarkable minor details at a point near 17ˢ preceding and 80″ north of *Eta*, which are changing rapidly."

Mr. Russell goes on to add, "I determined last year to examine this object, believing that when such a subject is under discussion, all who have the means of furnishing information should do so, and because the Sydney Refractor is, in defining and light-gathering power, nearer to the Reflector with which the Cape drawing was made than any which has since been directed to the object. . . . Unfortunately, in August last, *Eta Argûs* was too low down to admit of any satisfactory observations, and I was obliged to defer it to January this year; when I took every favourable opportunity of observing it, and completed the drawing early in March. The observations were taken after the object had attained an altitude of 50° up to 64°. Of the 108 stars in my list, I was able to identify 104 with those in the Cape list; of the remaining four, one, No. 25, is very small, and forms part of a triangle close to *Eta;* in 1834–8 it was probably hidden by the light of *Eta*."

Then, after a discussion upon the possible or probable change in some half-dozen stars, from the 6th to the 7th magnitude, he says,—"I would not, however, lay too much stress upon this until it is known whether all the 7th magnitudes in the Cape list were equal, for, if not, it may be only Nos. 71 and 105 have changed. As to the colours of the stars near *Eta*, they are in my estimation pale indeed compared with *κ Crucis;* and if in 1865 bright enough to merit Mr. Abbott's remark—'that although Sir John Herschel has not overdrawn the beauty of *κ Crucis*, the object *Eta Argûs* is much more superb'—they must have faded wonderfully since; for I only remarked colour in two, besides *Eta*, and both were red. Dr. Wright, who has examined both objects with an 8½-inch Browning reflector, failed to detect anything striking in those near *Eta*."

Mr. Russell then goes on to state that he carefully measured differences in R.A. and declination from *Eta* of fifty-four stars, and laid them down on a sheet of paper to a scale of twice the size of the Cape Monograph, filling in the Nebula from time to time by observations with the telescope. And he then further

states,—"The drawing was then reduced to its present size with proportional compasses, and afterwards compared with the object, under different states of the atmosphere, with Moon and without Moon, and corrected until it was deemed a faithful representation of it as seen now. . . . Examining it more in detail, it is seen that the mass in which the lemniscate (or dark enclosed space) is situated is in general outline very much the same as it was thirty years since, and in some of its marked features exactly the same; as, for instance, the definite outline beginning at $70^s—240''$, and running still, exactly as Sir John Herschel has described it, amongst some small stars; but round the border of the lemniscate great changes in the relative brightness of the different parts of the outline have taken place, and some slight changes in the outline itself; the greatest being at $17^s—100''$, where one point now occupies the place of two in the Cape drawing," &c.

Mr. Russell further speaks at some length of surprising changes in the "branches" of the Nebula beyond the lemniscate, viz., at $+40^s+600''$; and yet he states, "The star No. 11 (664 H) is just in the edge of the Nebula," precisely where Sir John has put it.

Towards the close of the paper, Mr. Russell says,—

"Taken as a whole, this object must have increased in brightness very much, for it can now be seen in full moonlight; and in 1834–8 it was at all times invisible to the naked eye. This fact, and the great similarity in outline between the Sydney and the Cape drawing, have inclined me to think, that if the same reflector could again be turned to this object the lemniscate would be found very little altered, and the apparent difference quite as much, perhaps more, in the increase of light at two points before indicated, as in the loss of light in other parts."

The copy of the *Sydney Morning Herald* from which the above extracts are made was sent in a letter from Mr. Russell to Collingwood, but arrived after Sir John's lamented decease; and having been favoured with the perusal of it, I have been induced to examine and compare the evidence both ways as to the supposed changes in this remarkable Nebula. My own conclusion, from a careful consideration of the various papers from which the above extracts are made, is, that we must require much more decisive evidence than herein exists, before we can admit that these alleged astounding changes have taken place. It is much to be regretted that Mr. Russell did not accompany his letter by a copy, more or less elaborate, of the drawing and stars he has so carefully laid down. It certainly would have been easy for any very moderately-skilled draughtsman to make one sufficiently accurate for the purpose. And also it is surprising that Mr. Russell should have omitted to give any description of the telescope he used, merely calling it the Sydney Refractor, without any mention of focus, aperture, or powers used. There are some other requirements which ought also to have been supplied before the hypothesis of even the alleged lesser changes can be allowed as

established. The Nebula, being circumpolar both at Hobart Town and Sydney, would (irrespective of the very great differences of altitude under which it was probably observed) present itself to the observers in every possible variety of position, as to *up or down*, and would I believe *therefore* occasion some differences of estimation of form and relative intensity of the different parts. Further, the variety of instruments used, generally of very inferior power to that of Sir John Herschel, and the general want of precision in most of the observations recorded, diminish greatly their respective weight. Again, the declension of magnitude of η, from nearly the 1st magnitude to the 6th or 7th, would of itself cause the respective brightness of the different parts of the surrounding nebula to be very differently estimated.

Under all these circumstances and conditions of this very conflicting testimony, I incline to the opinion that *no proof has yet been given of any change in the Nebula at all.* And, finally, if the subject be not already exhausted, I would refer to another letter from Capt. Herschel, dated 7th May, 1871, and inserted in the *Monthly Notice*, for June last.

Ray Lodge, Maidenhead,
19th July, 1871.

Theoretical Considerations respecting the Corona: Part II.
By R. A. Proctor, B.A. (Cambridge).

Before entering upon the consideration of the remaining portion of my subject, I propose briefly to discuss some questions which have been raised since the former part was written. I refer in particular to the meteoric theory of the Corona,—that is, the theory that a large proportion of the light of the Corona is due to the existence of countless hosts of meteors in the Sun's neighbourhood. I have mentioned already my belief that I had somewhat exaggerated the share which must be assigned to meteoric systems in accounting for coronal phenomena.* But this admission must

* In a paper by Prof. C. A. Young, of America, entitled "Note on the Spectrum of the Corona" (one of a series of highly suggestive and valuable contributions to the theory of the subject), he says, "Although I am not able to admit with Mr. Proctor that the whole explanation of the Corona is involved in the presence of such meteoric particles, yet it cannot be doubted that they are very numerous; and any that may come within 250,000 miles of the solar surface must become incandescent, &c." I cannot remember having on any occasion asserted that the whole explanation of the Corona is involved in the existence of meteoric systems near the Sun. I could quote many passages implying that I hold the contrary opinion. I may have written at times about the meteoric explanation as founded on a *vera causa* without discussing other *veræ causæ*; but it is not, therefore, to be inferred that I have doubted the reality of these others. I would notice in particular that I have always believed in the atmospheric nature of a portion of the light seen around the Sun during totality—even at the middle of the totality. In a paper in the *Monthly Notices* for March 1870, I note that the illumination of our atmosphere by the light of the prominences and sierra should result in "a faint diffused light diminishing

not be understood as having any reference whatever to opinions which I have publicly expressed. On the contrary, all my *statements* respecting the competence of the meteoric theory to account for the phenomena of the Corona, have been so guarded that I find nothing in them to modify. In speaking of a change of opinion, I have referred only to that view of the subject which I had been disposed to entertain in my own mind, not to any opinions which I had definitely enunciated. But I am not concerned at present to indicate how far, or how little, my own opinions have changed. Objections have recently been raised which would tend, if admitted, to invalidate the meteoric theory altogether as a means of explaining any portion of the coronal phenomena. These objections must be dealt with here, because they touch the very basis of the theory I am now considering. I refer to those remarks in Sir W. Thomson's able address at the meeting of the British Association in Edinburgh, in which he referred to the opinions he had once entertained respecting the meteoric origin of the solar heat.

The meteoric theory of the solar heat supply cannot be regarded as demonstrated, or even (considered as the sole account of the solar heat) as demonstrable; nor am I here anxious to support it in any way. But it is important to notice that the meteoric theory advanced and eventually abandoned by Sir W. Thomson is not the theory to which recent astronomical discoveries have pointed; nor can the reasoning which Sir W. Thomson has advanced as demonstrative against his own theory be urged with equal force (if with any force at all) against those views as to meteoric matter in the Sun's neighbourhood, which could alone be now advocated by the student of meteoric astronomy. It must be remembered that Sir W. Thomson's theory related to meteors circling chiefly within the orbit of the Earth, and he was led to abandon it "because Leverrier's researches on the motion of the planet *Mercury*, though giving evidence of a sensible influence attributable to matter circulating as a great number of small planets within *Mercury's* orbit, showed that the amount of matter which could possibly be assumed to circulate at any considerable

towards the neighbourhood of the Moon," and "extending over the Moon's disk (since it would illuminate the air between the observer and the Moon's body)." I need hardly observe that the same obvious reasoning which showed me that the prominences and sierra must produce this kind of illumination, convinced me also that the real solar Corona (extending beyond the highest prominences) must be added to the causes of this atmospheric illumination; but in a paper written expressly to show that there *is* such a solar Corona, I was not free to assume in the opening paragraphs the very point I sought to prove. As surely as the visibility of the prominences and sierra implies the existence of an atmospheric halo due to their light, so surely the visibility of a real solar Corona implies the visibility of an atmospheric halo due to the light of that Corona. That both haloes are very faint compared with the real solar Corona follows from the reasoning given in the same paper (*Monthly Notices*, vol. xxx., p. 142), that is (as Prof. Young and Dr. Balfour Stewart have since severally shown) from the darkness of the Moon's disk during totality, and from the faintness of the ordinary glare round the Sun by comparison with the light of the solar disk.

distance from the Sun must be very small;" therefore, "if the meteoric influx taking place at present is enough to produce any appreciable portion of the heat radiated away, it must be supposed to be from matter circulating round the Sun within very short distances of his surface. The density of this meteoric cloud would have to be supposed so great that comets could scarcely have escaped, as comets actually have escaped, showing no discoverable effects of resistance, after passing his surface within a distance equal to one-eighth* of his radius." But recent discoveries respecting meteors point to a totally different solution of the difficulty here considered. We are neither bound to show that the greater part of the meteoric matter available for the purpose in question is at any time present within the Earth's orbit, nor that the matter which is at any time so placed (or rather at a less distance from the Sun than our Earth) is the same matter which is similarly circumstanced at another time. All that we know respecting meteor-systems teaches us to regard them as, for the most part, travelling in very eccentric orbits, only a small part of each orbit lying at a less distance than the Earth from the Sun, and therefore only a small portion of each system being at any time nearer to the Sun than the Earth is. And further, we have every reason for believing that only a very small proportion of the meteoric systems travel near to the plane of the Earth's orbit. Among the meteoric orbits there is every variety of inclination; and, therefore, we have to deal with matter which is not collected in or near the plane of the ecliptic, but with matter completely enveloping the Sun on all sides. It is not a disk or very flat spheroid, but a sphere of meteoric matter, that we are concerned with in considering those portions of such matter which lie nearer to the Sun than our Earth does. It needs but a slight acquaintance with the laws of planetary motion to see that neither the motion of the Earth (and the inappreciable change in the length of the year), nor the motion of *Mercury* (and the slowness of the change in the position of his perihelion), can afford such significant evidence respecting the quantity of meteoric matter existing within given distances of the Sun, as was formerly supposed.

In like manner, another objection which Sir W. Thomson has urged against his former views loses much of its force when considered with reference to accepted meteoric theories. "Spectrum analysis," says Sir W. Thomson, "gives proof finally conclusive against the hypothesis that the Sun's heat is supplied dynamically from year to year by the influx of meteors. Each meteor circulating round the Sun must fall in along a very gradual spiral path, and before reaching the Sun must have been for a long time exposed to an enormous heating effect from his radiation when very near, and must thus have been driven into vapour before

* It should be, I conceive, "one-*fifth*," the comet referred to being that of the year 1843.

actually falling upon the Sun. Thus, if Mayer's hypothesis is correct, friction between vortices of meteoric vapours and the Sun's atmosphere must be the immediate cause of solar heat; and the velocity with which these vapours circulate round the equatorial parts of the Sun must amount to 435 kilomètres per second. The spectrum test of velocity applied by Lockyer showed but a twentieth part of this amount as the greatest observed relative velocity between different vapours in the Sun's atmosphere."

Now if this objection is sound as against the meteoric theory of the solar-heat supply, it is sound also as against the very existence of meteoric systems close to the Sun, a conclusion which very few will be disposed to admit after what has recently been discovered respecting meteors and comets. But I apprehend that the objection is obviated by precisely the same reasoning which is valid against the former objection. Undoubtedly if meteoric matter came in vortically around the equatorial parts of the Sun and in the direction of planetary motion, we might possibly expect to find some spectroscopic evidence of the existence of their vaporous substance, moving as it would with a velocity exceeding more than 200 times that due to the Sun's rotation. Even in this case it would appear venturesome to assert that the want of such evidence was "proof finally conclusive against" the existence of the meteoric vortices. For the meteoric matter being brought to rest (relatively to the Sun) by friction, there must be all possible rates of motion between 240 miles per second and rest with respect to the Sun's globe. The bright lines due to the vaporous meteors would, therefore, be widened so as to cover the space between their normal position and the positions due to the maximum velocity of 240 miles per second. That thus widened, and proportionately faint, they would be discernible as bright lines on the bright background of the solar spectrum, may be gravely doubted; that they *must* be so discernible may be safely denied. But the actual circumstances are very different even from those here considered. All the evidence recently obtained respecting meteors tends to show that those which approach the Sun neither "travel around his equatorial parts," nor move on a direct course, nor with a velocity that can be definitely assigned. Our Earth encounters more than 100 meteor systems (according to the observations of Heis, Alexander Herschel, and others), and though none of these systems probably pass near the Sun, yet we can infer from them what must be the characteristics of the millions on millions of meteoric systems which (according to all reasonable probability) belong to the solar system. We must conclude, then, that the systems which pass near the Sun are inclined in all possible directions to the solar equator, have every possible degree of eccentricity (and, therefore, every degree of velocity between 240 and 379 miles per second), and travel both in direct and retrograde courses. That the spectroscope would afford any evidence of the existence of these multifarious forms and degrees of motion, existing in systems which probably

include every variety of elementary constitution, and further modified by the effects of frictional resistance, so that every velocity down to relative rest must be included among the meteoric movements, is utterly improbable, to say the least.

It will be understood that if the objections here considered were valid, they would affect the theory that the Sun expels meteoric matter from his interior, as fatally as the general theory that meteoric systems are circulating in countless myriads around the Sun's globe. For the Sun is but one among the unnumbered millions of suns which exist throughout space, and if our Sun expels meteoric matter it must be inferred that the stars act in like manner. And objections against the existence of meteoric systems around the Sun—which cannot possibly be Sun-expelled—would be objections against star-expelled meteors, and so inferentially against the expulsion of meteoric matter from the Sun. It is on this account that I have thought it necessary to consider the objections dealt with above.

If any of the meteors which reach our Earth have really been expelled either from our own Sun or from his fellow-suns, we might expect that their structure, as well microscopic as chemical, would exhibit some signs of the circumstances under which these bodies had their origin.

Now as respects the microscopic structure of meteors, although we have much interesting information, and though some facts are known which seem scarcely explicable save on the strange theory I am considering, yet it must be admitted that there is much that is perplexing. Since I wrote the first part of this paper, I have had the opportunity of inspecting a large number of Mr. Sorby's singularly beautiful specimens, with his own instruments, and with the advantage of his own unrivalled experience to explain those facts which otherwise would have had little meaning to me. He also kindly gave me copies of all his papers on the subject, and these I have carefully studied. Space will not permit me to discuss here the various facts on which Mr. Sorby's reasoning and his (hypothetical) conclusions have been based. It must suffice for me to state, that while there remain many sources of perplexity in every part of the subject, he still considers the general conclusions which he published in 1864 as the most probable, and that, in fact, no other seems available. How far this conclusion is in accordance with the theory we are upon, the reader shall judge. "The most remote condition of which we have positive evidence," he wrote in 1864, "was that of small detached melted globules, the formation of which cannot be explained in a satisfactory manner, except by supposing that their constituents were originally in the state of vapour as they exist in the Sun." He found evidence that the meteors had been in the state of vapour while under enormous pressure, and "in mountain masses." It certainly seems difficult to understand where and how the substance of meteors could have been in this state,

save within an orb as intensely heated and as vast as our Sun and his fellow-suns.

The evidence from the chemical structure of meteors is even more striking.

If we consider the circumstances under which the meteors are supposed (according to this theory) to be expelled from the Sun or stars, and remark the evidence we have respecting the existence of hydrogen in other suns than ours, we shall see the probability that some among the meteors which reach us would show signs of having been once surrounded by intensely hot hydrogen, existing at an inconceivably vast pressure. For iron, which is so frequently present in meteoric masses, if solidified under such conditions, would condense within its substance a considerable proportion of hydrogen. Now we have evidence on excellent authority that meteoric iron contains a larger amount of occluded hydrogen than malleable iron can be impregnated with. The late Professor Graham examined a piece of the Lenarto meteor,—constituted, according to Werle's analysis, of 90·883 parts of iron, 8·450 parts of nickel, 0·665 of cobalt, and 0·002 of copper. When a volume of 5·78 cubic centimètres of this iron was heated to redness, "gas came off rather freely; namely, in 35 minutes 5·38 cubic centimètres, in the next 100 minutes 9·52, and in the next 20 minutes 1·63 cubic centimètres," in all, in rather more than 2½ hours, no less than 16·53 centimètres, or about 3 times the volume of the iron itself. "The first portion of the gas collected had a slight odour," says Professor Graham,* "but much less than the natural gases occluded by ordinary iron. It did not contain a trace of carbonic acid." The second portion of the gas collected (consisting of 9·52 cubic centimètres) gave of hydrogen 85·68 parts per cent, the rest consisting of nitrogen and carbonic oxide. "The Lenarto iron appears, therefore, to yield 2·85 times its volume of gas," says Professor Graham, "of which 86 per cent nearly is hydrogen, the proportion of carbonic oxide being so low as 4½ per cent." But "the gas occluded by iron from a carbonaceous fire is very different, the prevailing gas then being carbonic oxide. For comparison a quantity of clean horseshoe nails was submitted to a similiar distillation." This iron gave 2·66 times its volume of gas; the first portion collected contained only 35 per cent of hydrogen, 50·3 per cent being carbonic oxide, 7·7 per cent carbonic acid, and 7 per cent nitrogen; the second portion gave no carbonic acid, but 58 per cent of carbonic oxide, and only 21 per cent of hydrogen.

On these results Professor Graham reasons as follows:—"It has been found difficult to impregnate malleable iron with more than an equal volume of hydrogen under the pressure of our atmosphere. Now the meteoric iron (this Lenarto iron is remark-

* Not having Graham's original paper by me, I quote these passages from extracts in Mr. Mattieu Williams' *Fuel of the Sun*, where the theory of the expulsion of meteors from the Sun is enunciated and supported—on grounds, however, not always strictly in accordance with dynamical principles.

ably pure and malleable) gave up about three times that amount without being fully exhausted. The inference is that *the meteorite had been extruded from a dense atmosphere of hydrogen gas,* for which we must look beyond the light cometary matter floating about within the limits of our solar system. . . . Hydrogen has been recognized in the spectrum analysis of the light of the fixed stars by Messrs. Huggins and Miller. The same gas constitutes, according to the wide researches of Father Secchi, the principal element of a numerous class of stars, of which α *Lyræ* is the type. *The iron of Lenarto iron has no doubt come from such an atmosphere, in which hydrogen greatly prevailed. This meteorite may be looked upon as holding imprisoned within it, and bearing to us, the hydrogen of the stars.*"

Other circumstances relating to the Corona itself seem to require some such theory as that we are dealing with for their elucidation.

The coronal spectrum, although not by any means identical with the spectrum of the terrestrial aurora, show yet such a resemblance to this spectrum as to indicate that the Corona is in part due to a perpetual solar aurora. Such at least is the theory to which many profound reasoners have been led by the study of the coronal spectrum. But a difficulty had existed in determining how electrical action could be excited where we see the light of the Corona.* The theory we have been dealing with would remove this difficulty, for the rush of the erupted matter, even through the rare medium existing round the Sun, would produce precisely the effect which the coronal theory requires. The fact that one of the lines of the coronal spectrum belongs to the spectrum of iron may be regarded as supplying subsidiary evidence of some weight.

I have already referred to the fact that under close telescopic scrutiny the Corona presents close by the Sun an appearance as though countless thousands of jets were issuing from the photosphere. But it may be asked whether any direct evidence of an outrush of matter has ever been obtained? It might well happen that no such evidence was available; for, as I have mentioned, the actual volume of the erupted matter must be supposed to bear but the minutest possible proportion to the volume of the prominences. The swift motion of the erupted matter would not tend,

* When Prof. Reynolds exhibited (at the last meeting of the British Association) the very beautiful electrical Corona by which he illustrates the auroral theory of the Corona, Prof. Tait remarked that this theory had been rejected by men of science. It is difficult to understand on what grounds this remark was founded. I cannot find that any man of science has expressed an opinion adverse to the auroral theory. Dr. Balfour Stewart, General Sabine, and others, have used arguments respecting the prominences (before the nature of these was known) which may now be fairly applied to the Corona, while Prof. Young of America, Prof. Reynolds, and others in England, and several continental physicists, have spoken favourably of the auroral theory as directly applicable to the phenomena of the Corona. The noteworthy point is not, however, that there is such good authority in favour of the theory, but that not one man of science has definitely *expressed* an opinion adverse to it.

perhaps, to add to the difficulty of detection, because the effects of that motion at the Sun's distance would scarcely be appreciable, even in powerful telescopes. But it would be difficult to distinguish the erupted matter by its appearance, and as its light would give a continuous spectrum (owing to the enormous compression of the issuing jets), it would be wholly impossible to detect the existence of this matter by spectroscopic analysis. It may be questioned whether the brilliant flakes seen by Mr. Gilman in the large prominences visible during the Eclipse of 1869 can be regarded as in any way related to the subject we are upon. These flakes "stood out," he says, "as if totally unconnected from the rest of the prominence." But their size, as described and pictured by him, forbids us to believe that they could have been masses of erupted matter; though it is by no means impossible that they may have been clusters of many such masses resulting from volleyed discharges.

A phenomenon observed by Dr. Zöllner seems less questionably related to our subject. Observing the Sun on June 27, 1869, he noticed that as soon as he brought the slit of the spectroscope close to a certain part of the Sun's limb, *where the prominences were particularly long and bright*, brilliant linear flashes passed through the whole length of the dull spectrum, over the limb of the Sun, about three or four minutes' distance from the latter, "These flashes," he says, "passed over the whole of the spectrum in the field of view, and became so intense at a certain point of the Sun's limb as to produce the impression of a series of electrical discharges rapidly succeeding one another, and passing through the whole spectrum in straight lines. Mr. Vogel, who afterwards, for a short time, took part in these observations, found the same phenomenon at a different portion of the Sun's limb, *where protuberances also appeared.*" Zöllner remarks that "the phenomenon can be explained by the hypothesis that small intensely incandescent bodies moving near the surface of the Sun emit rays of all degrees of refrangibility, and produce flashes of a thread-like spectrum as their image passes before the slit of the spectroscope."

To these considerations may be added some which are connected with the aspect of the solar photosphere. For instance, the researches of De La Rue, Stewart, and Loewy, seem to prove that "the faculæ of a spot have been uplifted from the very area occupied by the spot, and have fallen behind from being thrown up into a region where the velocity of rotation is greater." This, of course, would correspond with what the theory we are considering would suggest. "And it may be noticed," here I quote from a paper of my own in *Fraser's Magazine*, for April 1871, "that, regarding spots as phenomena of eruption, that is, as *beginning* with eruption, we can find a reason for their occurrence being associated, as Mr. De La Rue and his colleagues believe, with the relative proximity of the planets. For eruptions and earthquakes on our own Earth, stable as its substance un-

doubtedly is by comparison with the Sun's, have been observed to occur more frequently when the Moon is in perigee; and Sir John Herschel has explained the predominance of active volcano and earthquake regions along shore-lines as depending on the seemingly insignificant changes due to tidal action. How much more, therefore, might we expect that the solar equilibrium would be disturbed by planetary action, when all that has been revealed respecting the Sun tends to show that the mightiest conceivable forces are always at work beneath his photosphere, one or other needing only (it may well be) the minutest assistance from without to gain a temporary mastery over its rivals. And if, as recent observations tend to show, the mightiest of the planets sympathises with solar action,—if when the Sun is most disturbed the belts of *Jupiter* are also subject (as of late and in 1860) to strange phenomena of change,—how readily do we find an explanation of what would otherwise seem so mysterious, when we remember that, as *Jupiter* disturbs the mighty mass of the Sun, so the Sun would reciprocally disturb the mass of the largest of his attendant orbs."

Brighton, September, 1871.

Observations of Saturn, Mars, &c. By Rev. J. Spear.

I am sorry I have not been able to record any observations of importance, except perhaps the error in the calculation of the occultation of ζ *Tauri*. I send, however, the following, which may, of course, be used as the Society may think fit.

Place of observation at the following dates mentioned herein:

Latitude	$30^{\circ}\ 42'\ 4''$
Longitude	$5^{h}\ 11^{m}\ 42^{s}$ E.
Elevation	6603 feet.

June 13th, 1870. Observed occultation of *Saturn* by the Moon. Definition *excellent*. The Moon passed steadily over the planet without causing any change of form, or giving *any* indication of the planet's light passing through an atmospheric medium. The planet, when near the Moon's limb, assumed a "sickly green hue," according to a note I made at the time, and which I saw corroborated in the *Monthly Notice* afterwards.

Nov. 9th, 6.30 A.M. Observed *Mars*. Phase gibbous. Snow and ice on the north pole, *intensely bright and glistening*. P. 240. Aperture 4½ inches. *Jupiter*, at the same time, appeared covered with belts, the equatorial belt, of ochreish colour (inclining to brown), was marked with spots and lines of bright light. There were several broken belts, both north and south; *one on* the south running in a diagonal direction.

16th. Observed two diagonal markings on the equatorial belt, also stiplings of bright cloud. The brightness of these floating clouds was very striking. The light earthy colour of the Great Belt seems to preclude the notion of the planet's still giving out any light of itself.

Dec. 6th. Observed the light yellow colour of the belts and satellites of *Jupiter.*

Dec. 8th. Watched for the occultation of ζ *Tauri*, Greenwich mean time of apparent conjunction in right ascension of Moon and Star, $5^h\ 0^m\ 3^s$. Limiting parallels, N. 90—N. 19. Lat. of place of observation, $30°\ 42'\ 4''$. No occurrence. The Moon passed at least $10'$ north of the star to the best of my judgment.

Telescope used $4\frac{1}{4}$ in. by Cooke and sons. Defining power excellent. By using a Barlow lens, and thus more than doubling its powers, it has separated ι *Arietis*, also shown the faint companion of κ *Geminorum*, test-objects for a 6-inch telescope.* "Good nights" are not very frequent, except in the cold season. Scintillation of stars within about 12° degrees of the horizon, is very considerable, even at my present elevation, about 7300 feet.

I fear there is nothing else in my notes worth communicating. I have not been able to obtain a good view of *Venus* lately, owing to the heavy fogs. The terminator appears irregular.

P.S.—Place of observation at present:—

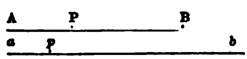
	°	′	″
Latitude	30	42	49
	h	m	s
Longitude	5	11	42
Elevation about 7300 feet.			

Churkrata, N. W. Provinces, Bengal.
July 12th, 1871.

Hyperbolic and Napierian Logarithms.
By Prof. A. D. Wackerbarth.

In almost all the elementary works—English, French, and German—which we make use of in teaching mathematics, the *natural* or *hyperbolic* logarithms are stated to be identical with the *Napierian.* But this is far from being really the case, and the difference is indeed fully stated in the introduction to Hutton's *Tables.* The highly gifted nobleman who invented logarithmic calculation defines his system thus:

Let a point p move with uniform velocity from a along the indefinite line ab, while another point P simultaneously moves from A along the finite line AB with a velocity proportional to PB, then is ab the logarithm of PB.



* This is dated Jan. 26th, 1871.

Let $AB=a$, $AP=x$, $ap=y$, and $BP=a-x=u$, and Log_N signify "Napierian logarithm of." We have, h and k being constants,

$$\frac{dy}{dt}=k \text{ and } \frac{dx}{dt}=h.(a-x)$$

therefore $y=k\,t$, no constant being required, and

$$-\mathrm{Log}_e(a-x)=h.t+C,$$

$$\text{therefore } -\mathrm{Log}_e\,a=C,$$

$$\mathrm{Log}_e\frac{a}{a-x}=h\,t, \quad\text{or}\quad \mathrm{Log}_e\frac{a}{u}=h.t$$

eliminating t,

$$y \text{ or } \mathrm{Log}_N\,u=\frac{k}{h}\frac{u}{a}\mathrm{Log}_e\frac{a}{u}=-\frac{k}{h}\mathrm{Log}_e\frac{u}{a}$$

As the constants $\frac{k}{p}$ and a are entirely arbitrary, Lord Napier assigned to them the value

$$a=\frac{k}{h}=10{,}000{,}000=10^7$$

and we accordingly have

$$\mathrm{Log}_N\,u=10^7.\mathrm{Log}_e\frac{10^7}{u}$$

$$=161\ 180\ 956{\cdot}508\ 5832-10^7\,\mathrm{Log}_e\,u.$$

This is sufficient to show the difference between the two systems, the one of which increases with the numbers, while the other decreases: for example:

$\mathrm{Log}_N\,1=161\ 180\ 956{\cdot}509\ 5832$	$\mathrm{Log}_e\ 1=0$
$\mathrm{Log}_N\,2=154\ 249\ 484{\cdot}703\ 9837$	$\mathrm{Log}_e\ 2=0{\cdot}693\ 1472$
$\mathrm{Log}_N\,10{,}000{,}000=0$	$\mathrm{Log}_e\ 10^7=16{\cdot}118\ 5957$

Errors of the Press in Wackerbarth's "Fem-ställiga Logarithm-tabelle."

Page 5, Log 2139, *for* 721 *read* 021.
28, Log 8827, *for* 681 *read* 581.
137, Cos 36° 54′, *for* 9·98, *read* 9·90.
223, Colog. Sun's Parallax, *for* 0·04, *read* 9·04.

A Chart of the Northern Hemisphere on an equal-surface projection, showing all the Stars in Argelander's Series of forty full-sheet Charts, 324,198 *in all, with a Key-Map on the same projection.* By R. A. Proctor, B.A., F.R.A.S. Manchester: Photographed and Published by A. Brothers, 1871.

In this Chart, within a circle 11 inches in diameter, are 324,198 minute dots, each representing a star, copied in with careful attention, both to position and size, from Argelander's series of star-charts: the chart thus showing the laws according to which the stars down to the 9–10th magnitude are distributed over the northern hemisphere.

The Chart being intended specially to show where the stars are richly and where sparsely strewn, the projection is such that equal spaces on the celestial vault are represented by equal spaces on the chart. The chart in the original scale formed a circle two feet in diameter: this was divided by meridians and parallels (radii and concentric circles) into 26,400 spaces, answering to corresponding spaces in Argelander's series: and into these spaces the stars were copied: the work of charting occupied almost exactly 400 hours, giving an average of 4½ seconds for each star. By means of Photography the work has been reproduced more satisfactorily than it could have been done by any engraving, however skilfully drawn: and the author notices that in this case, as in his Star-atlas, he has been most fortunate in obtaining the assistance of a fellow-astronomer (to whom such work is a labour of love).

Memoir by Prof. S. Newcomb on the Lunar Theory.

There is contained in the *Comptes Rendus* for 3 April, 1871, an account, by Prof. S. Newcomb, of a memoir, "Théorie des perturbations de la Lune qui sont dues à l'action des Planètes," presented by him to the Academy. The author seeks to avoid certain difficulties by regarding the perturbing force of the Sun as a principal force, and by proposing the problem as follows:

"*Assuming the solution of the problem of three bodies, to find the perturbations produced by the action of a fourth body, by the method of the variation of the arbitrary constants, making use of Lagrange's general formulæ.*"

Let m_1, m_2, m_3, m_4 be the masses of the Sun, Earth, Moon and Planet; ρ_1, ρ_2, ρ_3, the distances of the planet from the other three bodies; a_1, a_2, ... a_{18}, the arbitrary constants of the motion of the three bodies.

The disturbing function is

$$R = m_4\left(\frac{m_1}{\rho_1} + \frac{m_2}{\rho_2} + \frac{m_3}{\rho_3}\right).$$

B

R being expressed as a function of the arbitrary constants and of t; the variations are given by 18 differential equations of the form

$$(a_i, a_1)\frac{d a_1}{d t} + (a_i, a_2)\frac{d a_2}{d t} \dots + (a_i, a_{18})\frac{d a_{18}}{d t} = \frac{d \mathrm{R}}{d a_i}$$

where, as usual,

$$(a_i, a_j) = \Sigma m \left\{\frac{\partial(x, x')}{\partial(a_i, a_j)} + \frac{y}{\partial(a_i, a_j)} + \frac{\partial(z, z')}{\partial(a_i, a\ \ )}\right\}.$$

The number of combinations (a_i, a_j) is $= 153$: their direct calculation would be impracticable from its length, but it may be greatly simplified.

First, separating the six constants which determine the position of the centre of gravity of the three bodies from the other twelve, every combination (a, b) of one of the six constants with one of the twelve vanishes identically: and the combinations *inter se* of the six constants give simply the principle of the conservation of the centre of gravity: there remain to be considered only the 66 combinations of the 12 constants *inter se.*

The author takes—

a, e, γ, the constants usually spoken of as the mean distance of the Moon from the Earth, the excentricity of the orbit, and the inclination.

a', e', γ', the like constants for the motion of the common C. G. of the Earth and Moon about the Sun, γ' being the inclination of the ecliptic to the plane of X Y.

ϵ, ϖ, θ, ϵ', ϖ', θ', the mean longitude, longitude of perigee, and longitude of node for the Moon, and corresponding elements for the Sun or any six independent linear functions of these elements.

x, y, z, the co-ordinates of the Moon in reference to the Earth.

X, Y, Z, the co-ordinates of the Sun in reference to the common C. G. of the Moon and Earth.

The values of x, y, z, X, Y, Z, may be presented in the forms

$$x = \Sigma\, k \cos\,(i\,\epsilon + i^{\mathrm{I}}\varpi + i^{\mathrm{II}}\theta + i^{\mathrm{III}}\epsilon' + i^{\mathrm{IV}}\varpi' + i^{\mathrm{V}}\theta')$$
$$y = \Sigma\, k \sin\,(i\,\epsilon + i^{\mathrm{I}}\varpi + i^{\mathrm{II}}\theta + i^{\mathrm{III}}\epsilon' + i^{\mathrm{IV}}\varpi' + i^{\mathrm{V}}\theta')$$
$$z = \Sigma\, k' \sin\,(j\,\epsilon + j^{\mathrm{I}}\varpi + j^{\mathrm{II}}\theta + j^{\mathrm{III}}\epsilon' + j^{\mathrm{IV}}\varpi' + j^{\mathrm{V}}\theta')$$

X, Y, Z, being the like functions with K, K′, for k, k' (and k, k', K, K′, being functions of a, e, γ, a', e', γ').

It is shown that of the 66 terms (a_i, a_j) there are 30 which vanish, while the remaining 36 are the derivatives of 6 functions of a, e, γ, a', e', γ', with respect to these same constants: these 6 functions are represented by k_ϵ, k_ϖ, k_θ, $k_{\epsilon'}$, $k_{\varpi'}$, $k_{\theta'}$, and are formed as follows:—

1°. Each of the angles $i\,\epsilon + i'\,\varpi + \dots$, $j\,\epsilon + j'\,\varpi' + \dots$, being of the form A $+ b\,t$ where b is a function of a, e, γ, a', e', γ', we form for each term of each of the co-ordinates [x, y, z]* the product $\frac{m_2\, m_3}{m_2 + m_3}\, b\, k^2$, and for [each term of]* each of the co-ordinates

* I have added these insertions in [ ], which seem to me to be the meaning. —*Ed.*

X Y, Z, the product $\frac{m_1 (m_2 + m_3)}{m_1 + m_2 + m_3}$ B k^2. These products are called h_1, h_2, h_3, H_1, H_2, H_3: and the different values of h_1, h_2, are identical as well as those of H_1, H_2.

2°. Multiply each h by the corresponding coefficient of ι, and take the half sum of these products as well for x as for each of the other five co-ordinates of the Sun and Moon. This half-sum is called k_ι; and the like for $k_\varpi, k_\theta, k_\iota', k_\varpi', k_\theta'$. Then

$$(a, \iota) = -\frac{d k_\iota}{d a}, \ (e, \iota) = -\frac{d k_\iota}{d e} \ldots (\gamma', \iota) = \frac{d k_\iota}{d \gamma'}$$

$$(a, \varpi) = -\frac{d k_\varpi}{d a}, \ (e, \varpi) = -\frac{d k_\varpi}{d e} \ldots (\gamma', \varpi) = \frac{d k_\varpi}{d \gamma'}$$

[that is, 36 equations $(a, b) = \mp \frac{d k_b}{d a}$, if a is any one of the constants $a, e, \gamma, a', e', \gamma'$, and b any one of the constants $\iota, \varpi, \theta, \iota', \varpi', \theta'$, and the sign being — when the (a, b) are both unaccented or both accented, but + if one is accented and the other unaccented.—Ed.]

The equations for the variations thus are

$$\frac{d k_\iota}{d a}\frac{d a}{d t} + \frac{d k_\iota}{d e}\frac{d e}{d t} \ldots + \frac{d k_\iota}{d \gamma'}\frac{d \gamma'}{d t} = \frac{d \mathrm{R}}{d t}$$

$$\frac{d k_\varpi}{d a}\frac{d a}{d t} + \frac{d k_\varpi}{d e}\frac{d e}{d t} \ldots + \frac{d k_\varpi}{d \gamma'}\frac{d \gamma'}{d t} = \frac{d \mathrm{R}}{d t}$$

$$\frac{d k_\iota}{d a}\frac{d \iota}{d t} + \frac{d k_\varpi}{d a}\frac{d \varpi}{d t} \ldots + \frac{d k_{\theta'}}{d a}\frac{d \theta'}{d t} = -\frac{d \mathrm{R}}{d a}$$

or taking for variables k_ι, k_ϖ, &c., instead of $a, e, \gamma, a', e', \gamma'$, then the equations assume the canonical form,

$$\frac{d k_\iota}{d t} = \frac{d \mathrm{R}}{d \iota}, \ \frac{d k_\varpi}{d t} = \frac{d \mathrm{R}}{d \varpi}, \ldots$$

$$\frac{d \iota}{d t} = -\frac{d \mathrm{R}}{d k_\iota}, \ \frac{d \varpi}{d t} = -\frac{d \mathrm{R}}{d k_\varpi}, \ldots$$

The author remarks that the functions k_ι, k_ϖ, k_θ, have much analogy with those which M. Delaunay has called L, G, H. Taking as variables l, g, h, instead of ι, ϖ, θ, and forming the functions k_l, k_g, k_h, in like manner with k_ι, k_ϖ, k_θ, but attending only to the co-ordinates x, y, z, of the Moon, he obtains M. Delaunay's functions L, G, H, to a certain degree of approximation, but he has not attempted to demonstrate their rigorous identity.

The idea seems a very important one, and it may be anticipated that by thus starting from the problem of three bodies as if it were solved, and obtaining theorems in regard to determinate

functions (such as the above mentioned canonical elements) of the constants of the solution, some new light will be thrown on the original problem of three bodies. I have before remarked that in the ordinary investigations of Physical Astronomy, the problem treated of is not strictly that of three bodies, but a different and easier problem, that of disturbed elliptic motion.—Ed.

Instrument for Sale.

Transit Instrument for Sale: aperture $2\frac{1}{4}$ inches, with 5 wires. A very perfect specimen of modern workmanship, made by Troughton and Simms for an eminent astronomical baronet now deceased, at a cost of about 50*l.*; complete in box with the usual fittings, iron stand, price 30*l.* Apply to G. F. Chambers, Bickley, Kent, where the Instrument may be seen.

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AND OF

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DURING THE SAME PERIOD,

FORMING

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1871, *TO JUNE* 1872.

VOL. XXXII.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXII. *November* 10, 1871. No. 1.

WILLIAM LASSELL, Esq., F.R.S., President, in the Chair.

Josh. H. Freeman, Esq., Stratford House, Stratford;
R. Holford M. Bosanquet, Esq., St. John's College, Cambridge; and
Harry Taylor, Esq., Avenue Road, Regent's Park,

were balloted for and duly elected Fellows of the Society.

Note on the Construction of the Heavens, explanatory of a Chart of 324,198 *Stars.* By Richard A. Proctor, B.A. (Cambridge).

It is well known that, according to the theory of the universe enunciated by Sir W. Herschel in 1785,—that theory which is commonly, but erroneously, described as the outcome of his labours amid the star-depths,—the stars visible to the unaided eye are included within a space which bears an exceedingly minute proportion to the dimensions of the Galaxy itself. Herschel's original drawing of a section of the Galaxy—as conceived by him in 1785—is before me as I write. It extends across a folding sheet in a quarto volume. On the same scale, he says, the distance of the nearest fixed star would be no more than the 80th part of an inch, "so that probably all the stars which in the finest nights we are able to distinguish with the naked eye may be comprehended within a sphere," in the middle of this picture of our Galaxy, "*of less than half a quarter of an inch radius.*"

It was with reference to this view that in a paper called

"Notes on Star Streams," which appeared in the *Intellectual Observer* for August 1867, I pointed out that the stars of the first five orders of magnitude, only, show too marked a tendency to follow the direction of that great star-stream, the Milky Way, for the flat-disk theory to be admissible. I then noted,—and I am careful to mention the point, because objections have been urged which would imply that I had forgotten it,—that the first five orders of stars could give but very imperfect evidence *if* the accepted views about star distribution were just. But "it is on on that very fact," I added, "that I wish to dwell. If any connexion *does* appear between the configuration of our Galaxy, and the arrangement of stars which are assumed to be much nearer to us than the Milky Way, it will be obvious that we must somewhat modify our views."

I was not then aware that the astronomer Piazzi had noted the same fact,—though he had dealt rather with statistical evidence than with the more direct evidence I adduced from my star-charting. Nor was I then aware that Struve had followed up Piazzi's hint in the introduction to the *Catalogus Regiomontanus*, and that he had been led to precisely my own conclusion,—viz., that the observed distribution of the stars of the leading orders of magnitude is incompatible with the accepted or text-book theory of the Sidereal System. Still less was I then aware, though I had carefully read through all Sir W. Herschel's papers on the heavens, that in his later papers he virtually abandoned the theory of 1785. This fact had wholly escaped my notice, as I believe it escapes the notice of nearly all who give to those papers but a single perusal, however carefully that perusal may be made. It was only on a careful re-investigation of the whole series of Sir W. Herschel's papers (well worth and indeed requiring many readings) that I observed how completely his views changed some fifteen years or so after the enunciation of the theory of 1785. Somewhat later I found that Struve had been led, by a second reading of Herschel's papers, to a similar conclusion. For, in his *Etudes d'Astronomie Stellaire*, in which he extended his statistical inquiries to stars down to the 9th magnitude, he remarks that he "had been chiefly led to this ulterior discussion by a re-examination (*une nouvelle étude*) of the memoirs of Sir W. Herschel," which "has led me to the conviction," he adds, "that my actual ideas respecting the Milky Way are only opposed to Herschel's theory of 1785, but agree very well with the latter views (*sont extrêmement conformes aux vues postérieures*) of that great astronomer."

The researches I am now dealing with may be regarded as an extension of Struve's inquiry; only that whereas he employed a method of statistical research, I have preferred the method of actual charting to which I have adhered throughout these inquiries—whether I have dealt with stars or nebulæ. I believe that star-charting, appealing as it does directly to the eye, and presenting not only general features, but those minor details

which the statistician is obliged to neglect, is well worth the increased labour which it involves.

In order that the bearing of the present inquiry on Struve's, as well as on the Herschelian star-guages, may be rightly apprehended, it will be as well to note what Struve and the Herschels actually accomplished, the more so that mistaken notions are very prevalent on the subject.

For instance, with respect to the star-guagings, the opinion is not uncommonly entertained that the whole sphere of the heavens was surveyed or guaged by the Herschels. Many persons seem unaware of the fact that such a survey would have been quite impracticable even though these great astronomers had been able and willing to give their whole time to the work. When we remember that the labour actually bestowed by the Herschels on their star-guagings bore but a minute proportion to that which they bestowed on other and more difficult researches, we need not be surprised to find that the portion of the heavens actually guaged was exceedingly small compared with the whole surface of the celestial sphere. To afford an idea of the real extent of the Herschelian guages, the following illustration may be employed:—If the whole surface of the celestial sphere be represented by the surface of an ordinary chess-board, then all the guage-fields of Sir W. Herschel taken together (though 3400 in number) correspond to but about the fourth part of one of the squares; while the guage-fields surveyed by Sir John Herschel, taken together, correspond to somewhat less than a fifth part of one of the squares. Again, all the stars actually counted by Sir W. Herschel during his star-guaging amounted to about 90,000; those counted by Sir John Herschel to about 70,000: in all about 160,000. Now Struve estimates the total number of stars visible with the Herschelian guaging telescopes at upwards of 20 millions; so that the number actually counted is less than the 120th part of those which would have come into view in a complete survey. To this I may add that Sir W. Herschel, speaking of his 3400 guages, says that he regards them as "only an example to illustrate the spirit of the method."

Struve's latest and most extensive series of inquiries was applied to a list of 31,085 stars occupying a zone of the heavens between 15° north and 15° south of the equator, covering, therefore, a space equal to about the fourth part of the celestial sphere. By a series of ingenious inquiries into the number of stars probably missed in various parts of this zone, Struve virtually increased the number of stars he dealt with from about 31,000 to about 52,000.

The method he then adopted in dealing with these stars was simply this: he divided the great equatorial zone over which they were spread into hours of right ascension, and compared the numbers of stars in these divisions. He regarded these numbers as fairly indicating the richness of star-distribution *round the equator itself, compressing (as it were) a zone* 30° *wide into an*

indefinitely thin equatorial ring. Finally, *he converted this equatorial ring into an equatorial disc, by conceiving the stars of the various orders of magnitude to be distributed at corresponding distances from the centre of the celestial sphere.*

With all respect for the eminent abilities of the great Russian astronomer, I venture to point out that there are very grave objections to his method of research. The spaces into which he divided the heavens were inordinately large: each, in fact, extending 15° in right ascension and 30° in polar distance. To take averages for divisions so extensive must surely be regarded as unsatisfactory.

Moreover, the Galaxy, regarded by Struve as a zone of stellar concentration, runs obliquely across his wide equatorial zone, a circumstance obviously tending to render his results less instructive than they might otherwise have been. He notes this himself in accounting for the absence in his statistical results of all signs of the division of the Galaxy.

Lastly, as the main result of Struve's inquiries was to exhibit the want of uniformity in stellar distribution *over* the equatorial zone, his assumption of a uniform distribution *in the plane of* the equatorial disc, must be regarded as venturesome in the extreme.

As respects the construction of the chart which I now have the pleasure of exhibiting to this Society,* I need here say little, because I have elsewhere adverted to the points which seem of chief importance. I would note, however, that the pencilled divisions on this projection represent spaces on the sphere averaging in extent one 280th part, only, of those into which Struve divided his equatorial zone. Each space, also, was filled in, *not* according to the mere number of stars, but very carefully by an eye-draught from the corresponding space in Argelander's charts. That I might not be influenced by the natural tendency (as Sir John Herschel calls it) to "exaggerate any peculiarity which might at the moment seem a feature," each meridional sector, 5° wide, was covered over as soon as it was completed. The work when once commenced was carried on without intermission (except for rest and food), all other work being set aside. The necessity for this will be obvious when it is remembered that if the work had been left even for a few days only, the probability is that on returning to it I might have somewhat modified the scale of star-magnitudes, a circumstance which would have rendered all the work valueless. Even as it was, I found continual watchfulness necessary lest any change in the style of the work should be unwittingly admitted. It is not easy to work at a star-map for six or seven weeks in succession without

* Owing to a mistake on the part of the frame-maker, the original chart was not exhibited at the Meeting. Mr. Brothers obligingly supplied the defect by exhibiting a negative 8 inches in diameter. The original chart (25 inches in diameter) will be shown at the next Meeting.

changing the style or character of the charting; and I found it quite impossible to complete the chart in less than six or seven weeks. In fact, it is only necessary to compute the time required for marking in the stars to see that the work could not readily be compressed into a shorter interval of time. At the moderate rate of one minute for ten stars, 32,400 minutes, or 540 hours would be required; but the time actually occupied amounted only to 400 hours. I was pleased to find that along the line in which the last day's work was brought up to the first day's, no signs of any change in the style of mapping could be noticed. Had the star-magnitudes been enlarged or diminished in the interval, a sharp line of demarcation would unquestionably have been recognisable. As it is, I think I may defy the acutest observer to determine along what radial line the work began and ended.

So much being noted, in order that the trustworthiness of the chart may be sufficiently established, I will now briefly sum up the results to which the careful study of the chart appears to lead.

In the first place, Struve's general conclusion that the stars of the first nine or ten orders of magnitude are more densely aggregated along the galactic zone is abundantly justified.* But instead of a gradual increase of density such as his statistics suggested, we recognise in the chart a distinctly marked aggregation within those very regions of the heavens where the Milky Way is brightest to the eye. In other words, we have clear evidence that it is not towards a certain *zone* that the stars are gathered, but into those irregular cloudlike masses, those streams, projections, and interlacing branches which constitute the Milky Way, as it is actually presented on clear nights to our study.

Let me quote here—in correction of that account of the Milky Way which is too often presented in our text-books—the description which Prof. Nichol has given of that wonderful star region. "It is, indeed, only to the most careless glance," he justly says, "or when viewed through an atmosphere of imperfect transparency, that the Milky Way seems a continuous zone. Let the naked eye rest thoughtfully on any part of it, and if circumstances be favourable, it will stand out rather as an accumulation of patches and streams of light of every conceivable variety of form and brightness; now side by side; now heaped on each other; again spanning across dark spaces, intertwining, and forming a most curious and complex net-work; and at other times darting off into the neighbouring skies in branches of capricious length and shape, which gradually thin away and disappear."

* It is to be noticed that Argelander's magnitudes differ from those employed by Sir John Herschel. According to the nomenclature of the English astronomer my chart includes all stars down to the eleventh magnitude inclusive.

It will be seen that the aspect of the star-groups in the chart accords in the most significant manner with this description.

I would note, next, a circumstance which at first somewhat surprised me. The densely crowded regions in the chart show certain very well-marked projections in *Perseus* and *Auriga*, on the northern side of the Galaxy. Sir John Herschel, in his account of the Milky Way, mentions no such projections. But something in the aspect of these projecting masses seemed familiar to me so soon as I studied the complete chart. At length I was led to examine the Milky Way as depicted in the maps constructed by Sir John Lubbock. In my Gnomonic charts I had followed that figure of the Galaxy, and I had used the same delineation in a work now out of print, called the *Constellation Seasons*. On turning to my own drawings in these works, I found the very projections whose appearance had seemed familiar to me, as well as a long projection on the southern side of the Milky Way in *Perseus*, extending from *Algol* towards *Andromeda*. This projection also follows the direction of a declination-parallel, and its form is very clearly to be recognised in the chart. In the key-map I have shown these projections on opposite sides of the Milky Way in *Perseus*.

In the chart, however, we see these projections carried much further away from the main branch. We see, also, that from the other half of the Milky Way between *Cepheus* and *Aquila* there are similar extensions curving round on the northern side to a considerable distance from the main branch. One of these traverses *Lyra* and curves round almost to *Ursa Major*; another passes round from *Hercules* to *Corona*; while we note that the discontinuous branch which the naked eye recognises as extending from *Cygnus* to *Ophiuchus* is carried round towards *Boötes*.

Amongst these branching extensions there is, in places, so singular a tendency to agreement with declination-parallels, that the question may well suggest itself whether the peculiarity is not due to some feature in the construction of the maps—some bias, as it were, by which I might have been led to parallelize the star-grouping in this particular way; for plainly the north pole of the sky has no relation whatever to the construction of the stellar heavens. I am able to assert, however, with the utmost certainty of conviction, that nothing in the construction of the maps can explain the peculiarity. The original chart was most carefully divided into 26,400 spaces, each very small therefore, and the average number of stars to each space was but eighteen. These spaces were severally much smaller, both in length and breadth, than the cross-section of these branching extensions; so that even the most unpractised draughtsman, working in the most careless manner, could not have been so biassed that these curved branches could be thus explained away. As I have had some degree of practice in star-charting, and as, further, every portion of this chart was drawn as though it were forthwith to be subjected to the severest and most critical scrutiny, it will be understood how

utterly impossible it is that so marked a peculiarity can have the imagined explanation.*

Apart from this, the curved branches will be found, on a close scrutiny, not to agree exactly with the direction of declination-parallels; and all of them can be traced in Argelander's large charts, when these are carefully studied.

A circumstance of some interest is to be recognised in the fact that these branching extensions are found to lead, in almost every instance, towards regions of the heavens where many nebulæ exist.

The rich stellar region of *Orion*, as well as the *Hyades* and *Pleiades*, are shown to be intimately associated with the Milky Way.

In conclusion, I may remark that although the results to which this chart seems to point may appear altogether opposed to accepted views respecting the construction of the heavens, they accord exceedingly well with the enlarged views formed by Sir W. Herschel towards the close of his career as an observer,† and even better with the views towards which his later researches *pointed*. The mode of research I have adopted is altogether new, and it is one which seems more likely to lead to clear views of the construction of the heavens than any yet employed; for it presents before the eye at one view what hitherto has had to be inferred from tabulated figures. It is capable of a far wider extension, however; and I could wish that others who have more leisure than I can afford would aid in this interesting branch of inquiry. To such I would repeat the words by which Sir John Herschel, two years ago, encouraged me to fresh inquiries: "Be not deterred," he wrote on August 1, 1869, "from dwelling closely and consecutively on these inquiries, by any idea of their hopelessness which the objectors against 'paper astronomy' may entertain, or by the real slenderness of the material threads out of which any connected theory of the universe has at present to be woven."

Brighton, 8 *Nov*. 1871.

Extract of a Letter from E. J. Stone, Esq., to the Astronomer Royal, dated Royal Observatory, Cape of Good Hope, Sept. 30, 1871.

"I had hoped to have been able to forward the Catalogue for 1858 and 1859 by this mail, but my work has been nearly stopped through illness [of some of the Assistants]. The year 1859 is much richer in Southern stars than the other years. I hope to have the years 1857, 1858, and 1859 ready for press within a month."

* This does not preclude the possibility that there may be in places a tendency to "meridional" or "parallel" *graining*, so to speak; but of a much finer nature than the peculiarity referred to in the text, and in no way affecting the chart's real significance.

† I except the papers of 1817 and 1818.

On the Expression of Delaunay's l, g, h, in terms of his finally adopted Constants. By Prof. Cayley.

We have in Delaunay's lunar theory,—

l, the mean anomaly of the Moon,
g, the mean distance of perigee from ascending node,
h, the mean longitude of ascending node,

quantities which vary directly as the time, the coefficients of t, or values of $\frac{dl}{dt}, \frac{dg}{dt}, \frac{dh}{dt}$, being given in his *Théorie du Mouvement de la Lune*, vol. ii. pp. 237, 238. But these values are not expressed in terms of his constants a (or n), e, γ, finally adopted as explained p. 800, and it seems very desirable to obtain the expressions of l, g, h, in terms of these finally adopted constants: I have accordingly effected this transformation (which I found less laborious than I had anticipated). It will be convenient to imagine the a, n, e, γ of pp. 237, 238 replaced by A, N, E, Γ respectively. This being so, and writing m for the $\frac{n}{n'}$ of p. 800 we have, p. 800,—

$$
\begin{aligned}
A = a \Big\{ 1 &+ \left[-\frac{3}{4}m^2 - \frac{825}{256}m^3 \right] \frac{a^2}{a'^2} \\
&+ \left(-\frac{2}{3} + 3\gamma^2 - \frac{3}{4}e^2 - e'^2 - 2\gamma^4 + \frac{5}{2}\gamma^2 e^2 + \frac{9}{2}\gamma^2 e'^2 - \frac{1}{16}e^4 - \frac{9}{8}e^2 e'^2 - \frac{5}{4}e'^4 \right) m^2 \\
&+ \left(-\frac{9}{4}\gamma^2 - \frac{225}{16}e^2 + \frac{45}{8}\gamma^4 + \frac{81}{2}\gamma^2 e^2 - \frac{23}{4}\gamma^2 e'^2 + \frac{675}{128}e^4 - \frac{825}{16}e^2 e'^2 \right) m^3 \\
&+ \left(\frac{1705}{288} - \frac{1529}{64}\gamma^2 - \frac{14639}{256}e^2 + \frac{7469}{192}e'^2 \right) m^4 \\
&+ \left(\frac{787}{48} - \frac{9323}{256}\gamma^2 - \frac{227555}{1024}e^2 + \frac{7083}{32}e'^2 \right) m^5 \\
&+ \frac{5887}{162}m^6 \\
&+ \frac{29809}{432}m^7
\end{aligned}
$$

and hence calculating N from the formula $N^2 A^3 = n^2 a^3$, we find

$$N = n \Big\{ 1 + \left[\frac{9}{2}m^2 + \frac{2475}{[illegible]}m^3 \right] \frac{a^2}{a'^2}$$

$$+\left(1-\frac{9}{2}\gamma^2+\frac{9}{8}e^2+\frac{3}{2}e'^2+3\gamma^4-\frac{15}{4}\gamma^2e^2-\frac{27}{4}\gamma^2e'^2+\frac{3}{32}e^4+\frac{27}{16}e^2e'^2+\frac{15}{8}e'^4\right)m$$

$$+\left(\frac{27}{8}\gamma^2+\frac{675}{32}e^2-\frac{135}{16}\gamma^4-\frac{243}{4}\gamma^2e^2+\frac{69}{8}\gamma^2e'^2-\frac{2025}{256}e^4+\frac{2475}{32}e^2e'^2\right)m^3$$

$$+\left(-\frac{515}{64}+\frac{3627}{128}\gamma^2+\frac{44877}{512}e^2-\frac{7149}{128}e'^2\right)m^4$$

$$+\left(-\frac{787}{32}+\frac{30849}{512}\gamma^2+\frac{754665}{2048}e^2-\frac{21249}{64}e'^2\right)m^5$$

$$+\left(-\frac{13183}{192}\right)m^6$$

$$+\left(-\frac{20807}{144}\right)m^7.$$

$= n(1+Q)$ suppose $\therefore$

The values of E, Γ are given p. 800, but for the present purpose we only require E^2, and Γ^2 to the fifth order, viz. the values of these are at once found to be

$$E^2 = e^2\left(1+\frac{81}{64}m^2-\frac{2595}{128}m^3\right)$$

$$\Gamma^2 = \gamma^2\left(1+\frac{57}{64}m^2-\frac{129}{128}m^3\right)$$

whence also $E^4 = e^4$ and $\Gamma^4 = \gamma^4$.

The formulæ of pp. 237–238 now give

$$l = nt\left\{1+\left[-\frac{81}{32}m^2-\frac{2475}{512}m^3\right]\frac{a^2}{a'^2}+Q\right.$$

$$\left.+\left\{\begin{array}{l}\left(-\frac{7}{4}+\frac{21}{2}\gamma^2-\frac{3}{4}e^2-\frac{21}{8}e'^2+\frac{33}{4}\gamma^4-\frac{39}{8}\gamma^2e^2+\frac{63}{4}\gamma^2e'^2-\frac{9}{8}e^2e'^2-\frac{105}{32}e'^4\right)m^2\\ +\left(\frac{1197}{128}\gamma^2-\frac{243}{256}e^2\right)m^4\\ +\left(-\frac{2709}{256}\gamma^2+\frac{7785}{512}e^2\right)m^5\end{array}\right\}(1+Q)^{-}\right.$$

On the Expression of Delaunay's l, g, h, in terms of his finally adopted Constants. By Prof. Cayley.

We have in Delaunay's lunar theory,—

l, the mean anomaly of the Moon,
g, the mean distance of perigee from ascending node,
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quantities which vary directly as the time, the coefficients of t, or values of $\frac{dl}{dt}, \frac{dg}{dt}, \frac{dh}{dt}$, being given in his *Théorie du Mouvement de la Lune*, vol. ii. pp. 237, 238. But these values are not expressed in terms of his constants a (or n), e, γ, finally adopted as explained p. 800, and it seems very desirable to obtain the expressions of l, g, h, in terms of these finally adopted constants: I have accordingly effected this transformation (which I found less laborious than I had anticipated). It will be convenient to imagine the a, n, e, γ of pp. 237, 238 replaced by A, N, E, Γ respectively. This being so, and writing m for the $\frac{n}{n'}$ of p. 800 we have, p. 800,—

$$\begin{aligned}
A = a \Big\{ & 1 + \left[-\frac{3}{4}m^2 - \frac{825}{256}m^3\right]\frac{a^2}{a'^2} \\
& + \left(-\frac{2}{3} + 3\gamma^2 - \frac{3}{4}e^2 - e'^2 - 2\gamma^4 + \frac{5}{2}\gamma^2 e^2 + \frac{9}{2}\gamma^2 e'^2 - \frac{1}{16}e^4 - \frac{9}{8}e^2 e'^2 - \frac{5}{4}e'^4\right)m^2 \\
& + \left(-\frac{9}{4}\gamma^2 - \frac{225}{16}e^2 + \frac{45}{8}\gamma^4 + \frac{81}{2}\gamma^2 e^2 - \frac{23}{4}\gamma^2 e'^2 + \frac{675}{128}e^4 - \frac{825}{16}e^2 e'^2\right)m^3 \\
& + \left(\frac{1705}{288} - \frac{1529}{64}\gamma^2 - \frac{14639}{256}e^2 + \frac{7469}{192}e'^2\right)m^4 \\
& + \left(\frac{787}{48} - \frac{9323}{256}\gamma^2 - \frac{227555}{1024}e^2 + \frac{7083}{32}e'^2\right)m^5 \\
& + \frac{5887}{162}m^6 \\
& + \frac{29809}{432}m^7
\end{aligned}$$

and hence calculating N from the formula $N^2 A^3 = n^2 a^3$, we find

$$\Big\{ \Big[0 \; . \; 2475 \; . \Big] e^2$$

$$+\left(1-\frac{3}{2}\gamma^2+\frac{9}{8}e^2+\frac{3}{2}e'^2+3\gamma^4-\frac{15}{4}\gamma^2e^2-\frac{27}{4}\gamma^2e'^2+\frac{3}{32}e^4+\frac{27}{16}e^2e'^2+\frac{15}{8}e'^4\right)m$$
$$+\left(\frac{27}{8}\gamma^2+\frac{675}{32}e^2-\frac{135}{16}\gamma^4-\frac{243}{4}\gamma^2e^2+\frac{69}{8}\gamma^2e'^2-\frac{2025}{256}e^4+\frac{2475}{32}e^2e'^2\right)m^3$$
$$+\left(-\frac{515}{64}+\frac{3627}{128}\gamma^2+\frac{44877}{512}e^2-\frac{7149}{128}e'^2\right)m^4$$
$$+\left(-\frac{787}{32}+\frac{30849}{512}\gamma^2+\frac{754665}{2048}e^2-\frac{21249}{64}e'^2\right)m^5$$
$$+\left(-\frac{13183}{192}\right)m^6$$
$$+\left(-\frac{20807}{144}\right)m^7.$$

$= n(1+Q)$ suppose $\therefore$

The values of E, Γ are given p. 800, but for the present purpose we only require E^2, and Γ^2 to the fifth order, viz. the values of these are at once found to be

$$E^2=e^2\left(1+\frac{81}{64}m^2-\frac{2595}{128}m^3\right)$$
$$\Gamma^2=\gamma^2\left(1+\frac{57}{64}m^2-\frac{129}{128}m^3\right)$$

whence also $E^4=e^4$ and $\Gamma^4=\gamma^4$.

The formulæ of pp. 237–238 now give

$$l=nt\left\{1+\left[-\frac{81}{32}m^2-\frac{2475}{512}m^3\right]\frac{a^2}{a'^2}+Q\right.$$
$$+\left\{\begin{array}{l}\left(-\frac{7}{4}+\frac{21}{2}\gamma^2-\frac{3}{4}e^2-\frac{21}{8}e'^2+\frac{33}{4}\gamma^4-\frac{39}{8}\gamma^2e^2+\frac{63}{4}\gamma^2e'^2-\frac{9}{8}e^2e'^2-\frac{105}{32}e'^4\right)m^3\\+\left(\frac{1197}{128}\gamma^2-\frac{243}{256}e^2\right)m^4\\+\left(-\frac{2709}{256}\gamma^2+\frac{7785}{512}e^2\right)m^5\end{array}\right\}(1+Q)^{-}$$

$$
+\left\{\begin{aligned}&\left(-\frac{225}{32}+\frac{81}{4}\gamma^2-\frac{675}{64}e^2-\frac{825}{32}e'^2-\frac{243}{4}\gamma^4+\frac{1863}{32}\gamma^2e^2+\frac{629}{8}\gamma^2e'^2+\frac{2025}{256}e^4-\frac{2475}{64}e^2e'^2\right)m^3\\&\qquad+\left(\frac{4617}{256}\gamma^2-\frac{54675}{4096}e^2\right)m^3\end{aligned}\right\}(1+Q)^{-2}
$$

$$
+\left(-\frac{3265}{128}+\frac{3345}{32}\gamma^2-\frac{7089}{256}e^2-\frac{48225}{256}e'^2\right)m^4(1+Q)^{-3}
$$

$$
+\left(-\frac{243925}{2048}+\frac{175425}{256}\gamma^2-\frac{167835}{2048}e^2-\frac{1502265}{1024}e'^2\right)m^5(1+Q)^{-4}
$$

$$
+\left(-\frac{12626759}{24576}\right)m^6(1+Q)^{-}
$$

$$
+\left(-\frac{1365131021}{589824}\right)m^7(1+Q)^{-7}
$$

(Observe that writing herein $Q=0$, and omitting the terms in m^4 and m^5 in the coefficient of $(1+Q)^{-1}$, and the term in m^5 in the coefficient of $(1+Q)^{-2}$, we have the original formula of p. 237)

$$
g=nt\left\{\begin{aligned}&\left[\frac{45}{16}m^2+\frac{585}{32}m^3\right]\frac{a^2}{a'^2}\\
&+\left\{\begin{aligned}&\left(\frac{3}{2}-\frac{15}{2}\gamma^2+\frac{9}{8}e^2+\frac{9}{4}e'^2-\frac{45}{4}\gamma^4+15\gamma^2e^2-\frac{45}{4}\gamma^2e'^2-\frac{27}{64}e^4+\frac{27}{16}e^2e'^2+\frac{45}{16}e'^4\right)m^2\\&\qquad+\left(-\frac{855}{128}\gamma^2+\frac{729}{512}e^2\right)m^4\\&\qquad+\left(\frac{1935}{256}\gamma^2-\frac{23355}{1024}e^2\right)m^5\end{aligned}\right\}(1+Q)^{-1}\\
&+\left\{\begin{aligned}&\left(\frac{27}{4}-\frac{351}{16}\gamma^2-\frac{297}{64}e^2+\frac{401}{16}e'^2+\frac{135}{2}\gamma^4-\frac{1053}{32}\gamma^2e^2-\frac{1297}{16}\gamma^2e'^2+\frac{675}{256}e^4-\frac{1079}{64}e^2e'^2\right)m^3\\&\qquad+\left(-\frac{20007}{1024}\gamma^2-\frac{24057}{4096}e^2\right)m^5\end{aligned}\right\}(1+Q)^{-2}\\
&+\left(\frac{1995}{64}-\frac{7989}{64}\gamma^2-\frac{9969}{256}e^2+\frac{29535}{128}e'^2\right)m^4(1+Q)^{-3}\\
&+\left(\frac{17709}{[illegible]}-\frac{376653}{512}\gamma^2-\frac{440787}{2048}e^2+\frac{883245}{512}e'^2\right)m^5(1+Q)^{-4}\end{aligned}\right.
$$

$$+\frac{62329307}{24576}m^7(1+Q)^{-6}$$

(where writing $Q=0$, and omitting the terms in m^4 and m^5 in the coefficient of $(1+Q)^{-1}$, and the term in m^5 in the coefficient of $(1+Q)^{-2}$, we have the original formula of p. 237). And

$$\begin{aligned}h=nt\Big\{&\left[-\frac{45}{32}m^3-\frac{1935}{512}m^5\right]\frac{a^2}{a'^2}\\
&+\left\{\begin{aligned}&\left(-\frac{3}{4}+\frac{3}{2}\gamma^2-\frac{3}{2}e^2-\frac{9}{8}e'^2-\frac{51}{8}\gamma^2e^2+\frac{9}{4}\gamma^2e'^2+\frac{21}{64}e^4-\frac{9}{4}e^2e'^2-\frac{45}{32}e'^4\right)m^2\\
&\qquad+\left(\frac{171}{128}\gamma^2-\frac{243}{128}e^2\right)m^4\\
&\qquad+\left(-\frac{387}{256}\gamma^2+\frac{7785}{256}e^2\right)m^5\end{aligned}\right\}(1+Q)^{-1}\\
&+\left\{\begin{aligned}&\left(\frac{9}{32}-\frac{27}{16}e^2-\frac{189}{32}\gamma^2+\frac{23}{32}e'^2+\frac{27}{16}\gamma^4+\frac{567}{16}\gamma^2e^2-\frac{99}{16}\gamma^2e'^2-\frac{675}{256}e^4-\frac{349}{16}e^2e'^2\right)m^3\\
&\qquad+\left(-\frac{1539}{1024}\gamma^2-\frac{15309}{2048}e^2\right)m^5\end{aligned}\right\}(1+Q)^{-2}\\
&+\left(\frac{177}{128}-\frac{195}{64}\gamma^2-\frac{699}{32}e^2+\frac{2685}{256}e'^2\right)m^4(1+Q)^{-3}\\
&+\left(\frac{10949}{2048}-\frac{6369}{512}\gamma^2-\frac{133839}{1024}e^2+\frac{75759}{1024}e'^2\right)m^5(1+Q)^{-4}\\
&+\frac{467977}{24576}m^6(1+Q)^{-5}\\
&+\frac{26983045}{589824}m^7(1+Q)^{-6}\end{aligned}$$

(where writing $Q=0$, and omitting the terms in m^4 and m^5 in the coefficient of $(1+Q)^{-1}$, and the term in m^5 in the coefficient of $(1+Q)^{-2}$, we have the original formula of p. 238). We hence have

$$\begin{aligned}l&=nt\{A+(1+B)Q+CQ^2\},\\
&=nt\{A+Q+BQ+CQ^2\},\\
g&=nt\{A'+B'Q+C'Q^2\},\\
h&=nt\{A''+B''Q+C''Q^2\},\end{aligned}$$

where (omitting the terms in $\frac{a^2}{a'^2}$)

$$\begin{aligned}
A = 1 &+ \left(-\frac{7}{4} + \frac{21}{2}\gamma^2 - \frac{3}{4}e^2 - \frac{21}{8}e'^2 + \frac{33}{4}\gamma^4 - \frac{39}{8}\gamma^2 e^2 + \frac{63}{4}\gamma^2 e'^2 - \frac{9}{8}e^2 e'^2 - \frac{109}{32}e'^4\right) m^2 \\
&+ \left(-\frac{225}{32} + \frac{81}{4}\gamma^2 - \frac{675}{64}e^2 - \frac{825}{32}e'^2 - \frac{243}{4}\gamma^4 + \frac{1863}{32}\gamma^2 e^2 + \frac{629}{8}\gamma^2 e'^2 + \frac{2025}{256}e^4 - \frac{105}{32}e'^4\right) m^3 \\
&+ \left(-\frac{3265}{128} + \frac{14577}{128}\gamma^2 - \frac{1833}{64}e^2 - \frac{48225}{256}\right) m^4 \\
&+ \left(-\frac{243925}{2048} + \frac{177333}{256}\gamma^2 - \frac{328065}{4096}e^2 - \frac{1502265}{1024}e'^2\right) m^5 \\
&+ \left(-\frac{12626759}{24576}\right) m^6 \\
&+ \left(-\frac{1365131021}{589824}\right) m^7. \\
B = &\ \left(\frac{7}{4} - \frac{21}{2}\gamma^2 + \frac{3}{4}e^2 + \frac{21}{8}e'^2\right) m^2 \\
&+ \left(\frac{225}{16} - \frac{81}{2}\gamma^2 + \frac{675}{32}e^2 + \frac{825}{16}e'^2\right) m^3 \\
&+ \frac{9795}{128} m^4 \\
&+ \frac{243925}{512} m^5. \\
C = &- \frac{7}{4} m^2 \\
&- \frac{675}{32} m^3. \\
A' = &\ \left(\frac{3}{2} - \frac{15}{2}\gamma^2 + \frac{9}{8}e^2 + \frac{9}{4}e'^2 - \frac{45}{4}\gamma^4 + 15\gamma^2 e^2 - \frac{45}{4}\gamma^2 e'^2 - \frac{27}{16}e^4 + \frac{27}{16}e^2 e'^2 + \frac{45}{16}e'^4\right) m^2 \\
&+ \left(\frac{27}{4} - \frac{351}{16}\gamma^2 - \frac{297}{64}e^2 + \frac{401}{16}e'^2 + \frac{135}{2}\gamma^4 - \frac{1053}{32}\gamma^2 e^2 - \frac{1297}{16}\gamma^2 e'^2 + \frac{675}{256}e^4 - \frac{1079}{64}e^2 e'^2\right) m^3 \\
&+ \left(\frac{1995}{64} - \frac{16833}{128}\gamma^2 - \frac{19209}{512}e^2 + \frac{29535}{128}e'^2\right) m^4
\end{aligned}$$

$$+\left(\frac{17709}{128}-\frac{785573}{1024}\gamma^2-\frac{999051}{4096}e^2+\frac{883245}{512}e'^2\right)m$$

$$+\frac{2431349}{4096}m^6$$

$$+\frac{62329307}{24576}m^7.$$

$$B'=\left(-\frac{3}{2}+\frac{15}{2}\gamma^2-\frac{9}{8}e^2-\frac{9}{4}e'^2\right)m^2$$

$$+\left(-\frac{27}{2}+\frac{351}{8}\gamma^2+\frac{297}{32}e^2-\frac{401}{8}e'^2\right)m^3$$

$$-\frac{5985}{64}m^4$$

$$-\frac{17709}{32}m^5$$

$$C=\frac{3}{2}m^2$$

$$+\frac{81}{4}m^3.$$

$$A''=\left(-\frac{3}{4}+\frac{3}{2}\gamma^2-\frac{3}{2}e^2-\frac{9}{8}e'^2-\frac{51}{8}\gamma^2e^2+\frac{9}{4}\gamma^2e'^2+\frac{21}{64}e^4-\frac{9}{4}e^2e'^2-\frac{45}{32}e'^4\right)m^2$$

$$+\left(\frac{9}{32}-\frac{27}{16}\gamma^2-\frac{189}{32}e^2+\frac{23}{32}e'^2+\frac{27}{16}\gamma^4+\frac{567}{16}\gamma^2e^2-\frac{99}{16}\gamma^2e'^2-\frac{675}{256}e^4-\frac{349}{16}e^2e'^2\right)m^3$$

$$+\left(\frac{177}{128}-\frac{219}{128}\gamma^2-\frac{3039}{128}e^2+\frac{2685}{256}e'^2\right)m^4$$

$$+\left(\frac{10949}{2048}-\frac{15825}{1024}\gamma^2-\frac{220707}{2048}e^2+\frac{75759}{1024}e'^2\right)m^5$$

$$+\frac{467977}{24576}m^6$$

$$+\frac{26983045}{589824}m^7$$

$$
\begin{aligned}
B'' = &+\left(\tfrac{3}{4}-\tfrac{3}{2}\gamma^2+\tfrac{3}{2}e^2+\tfrac{9}{8}e'^2\right)m^2\\
&+\left(-\tfrac{9}{16}+\tfrac{27}{8}\gamma^2+\tfrac{189}{16}e^2-\tfrac{23}{16}e'^2\right)m^3\\
&-\tfrac{531}{128}m^4\\
&-\tfrac{10949}{512}m^5,\\
C'' = &-\tfrac{3}{4}m^2\\
&+\tfrac{27}{32}m^3.
\end{aligned}
$$

And in the terms B Q, B′ Q, B″ Q, we have

$$
\begin{aligned}
Q = &\left(1-\tfrac{9}{2}\gamma^2+\tfrac{9}{8}e^2+\tfrac{3}{2}e'^2\right)m^2\\
&+\left(\tfrac{27}{8}\gamma^2+\tfrac{675}{32}e^2\right)m^3\\
&-\tfrac{515}{64}m^4\\
&-\tfrac{787}{32}m^5,
\end{aligned}
$$

and in the terms C Q^2, C′ Q^2, C″ Q^2, simply $Q^2 = m^4$. Hence finally the required values of l, g, h, are

$$
\begin{aligned}
l = nt\Big\{1 &+ \left[-\tfrac{45}{32}m^2-\tfrac{7425}{512}m^3\right]\tfrac{a^2}{a'^2}\\
&+\left(-\tfrac{3}{4}+6\gamma^2+\tfrac{3}{8}e^2-\tfrac{9}{8}e'^2+\tfrac{45}{4}\gamma^4-\tfrac{69}{8}\gamma^2e^2+9\gamma^2e'^2+\tfrac{3}{32}e^4+\tfrac{9}{16}e^2e'^2-\tfrac{45}{32}e'^4\right)m^2\\
&+\left(-\tfrac{225}{32}+\tfrac{189}{8}\gamma^2+\tfrac{675}{64}e^2-\tfrac{825}{32}e'^2-\tfrac{1107}{16}\gamma^4-\tfrac{81}{32}\gamma e^2+\tfrac{349}{4}\gamma^2e'^2+\tfrac{2475}{64}e^2e'^2\right)m^3\\
&+\left(-\tfrac{4071}{128}+\tfrac{15852}{128}\gamma^2+\tfrac{31605}{512}e^2-\tfrac{61179}{256}e'^2\right)m^4
\end{aligned}
$$

$$+\left(-\frac{[illegible]}{24576}\right)m^{6}$$

$$+\left(-\frac{1273925965}{589824}\right)m^{7},$$

$$g=nt\left\{\quad\left[\frac{45}{16}m^{2}+\frac{585}{32}m^{3}\right]\frac{a^{2}}{a'^{2}}\right.$$

$$+\left(\frac{3}{2}-\frac{15}{2}\gamma^{2}+\frac{9}{8}e^{2}+\frac{9}{4}e'^{2}-\frac{45}{4}\gamma^{4}+15\gamma^{2}e^{2}-\frac{45}{4}\gamma^{2}e'^{2}-\frac{27}{64}e^{4}+\frac{27}{16}e^{2}e'^{2}+\frac{45}{16}e'^{4}\right)m^{2}$$

$$+\left(\frac{27}{4}-\frac{351}{16}\gamma^{2}-\frac{297}{64}e^{2}+\frac{401}{16}e'^{2}+\frac{135}{2}\gamma^{4}-\frac{1053}{32}\gamma^{2}e^{2}-\frac{1297}{16}\gamma^{2}e'^{2}+\frac{675}{256}e^{4}-\frac{1079}{64}e^{2}e'^{2}\right)m^{3}$$

$$+\left(\frac{1899}{64}-\frac{15009}{128}\gamma^{2}-\frac{20649}{512}e^{2}-\frac{28959}{128}e'^{2}\right)m^{4}$$

$$+\left(\frac{15981}{128}-\frac{663621}{1024}\gamma^{2}-\frac{1152843}{4096}e^{2}+\frac{847213}{512}e'^{2}\right)m^{5}$$

$$+\frac{2103893}{4096}m^{6}$$

$$+\frac{52802843}{24576}m^{7},$$

$$h=nt\left\{\quad\left[-\frac{45}{32}m^{2}-\frac{1935}{512}m^{3}\right)\frac{a^{2}}{a'^{2}}\right.$$

$$+\left(-\frac{3}{4}+\frac{3}{2}\gamma^{2}-\frac{3}{2}e^{2}-\frac{9}{8}e'^{2}-\frac{51}{8}\gamma^{2}e^{2}+\frac{9}{4}\gamma^{2}e'^{2}+\frac{21}{64}e^{4}-\frac{9}{4}e^{2}\,{}'^{2}-\frac{45}{32}e'^{4}\right)m^{2}$$

$$+\left(\frac{9}{32}-\frac{27}{16}\gamma^{2}-\frac{189}{32}e^{2}+\frac{23}{33}e'^{2}+\frac{27}{16}\gamma^{4}+\frac{567}{16}\gamma^{2}e^{2}-\frac{99}{16}\gamma^{2}e'^{2}-\frac{675}{256}\,{}^{4}-\frac{349}{16}e^{2}e'^{2}\right)m^{3}$$

$$+\left(\frac{273}{128}-\frac{843}{128}\gamma^{2}-\frac{2739}{128}e^{2}+\frac{3261}{256}e'^{2}\right)m^{4}$$

$$+\left(\frac{9797}{2048}-\frac{7185}{1024}\gamma^{2}-\frac{165411}{2048}e^{2}+\frac{73423}{1024}e'^{2}\right)m^{5}$$

$$+\frac{199273}{24576}m^{6}$$

$$+\frac{6657733}{589824}m^{7},$$

which values satisfy, as they should do, the equation $l + g + h = nt$. I recall that the precise signification of the constants is as follows:—n is the coefficient of t in the expression of the Moon's longitude in terms of the time, a the corresponding elliptic value of the mean distance ($n^2 a^3 =$ sum of masses), e the excentricity, such that in the expression of the longitude the coefficient of the leading term of the equation of the centre has its elliptic value

$$= 2e - \frac{1}{4}e^3 + \frac{5}{96}e^5$$

and γ the sine of the half-inclination, such that in the expression of the latitude the coefficient of the leading term has its elliptic value

$$= 2\gamma - 2\gamma e^2 - \frac{1}{4}\gamma^3 + \frac{7}{32}\gamma e^4 + \frac{1}{4}\gamma^3 e^2 - \frac{5}{144}\gamma e^6$$

n', a' are the mean motion and mean distance of the Sun, $m = \frac{n'}{n}$, and e is the excentricity of the Sun's orbit, considered as constant.

Extract of a Letter from Dr. B. A. Gould to the Astronomer Royal, dated Cordoba, Sept. 30, 1871.*

"Our work here has prospered as well as is consistent with the absolute impossibility of beginning it. That is to say, although the Observatory is, up to this present day, twelve and a half months after my arrival, not yet sufficiently complete to enable the instrumental observations for which I came here to begin, the Uranometry has more than fulfilled my most earnest hopes, and has enabled us to work very hard to useful purpose. Between the South Pole and $+10^\circ$ North Declination I have a catalogue of over 7100 stars visible to the naked eye on good nights, reaching to the magnitude which I am at present provisionally designating as 6·6 mag. All these stars have either been identified in the Catalogues or found not to be contained therein, and all their places have been referred to the mean equinox of our adopted epoch. And the magnitudes of all have already been so fully studied that I believe the mean error of the determinations cannot well exceed 0·1 mag. There are relatively few which have not been determined at least twice, and for the majority there are at least four determinations of magnitude, and of course by more than one observer in every case. Another month ought to complete the work of cataloguing and identifying, and of excluding such as, although they have been seen on favourable nights, are below the inferior limit which I have fixed

* Argentine Republic, about Lat. 31° S.—*Ed.*

upon. Then the work of mapping, of determining the places with greater accuracy, and of revising, will be a matter of simple routine.

"Of course a few variables have come to light, the observations on which I hope soon to have arranged in form for transmission to the *Astronomische Nachrichten* or the Royal Astronomical Society for publication. One has a period of only 21 hours, and though varying through less than a single magnitude, is so near the limit of unaided vision that it oscillates between visibility and invisibility. This is in *Triangulum Australis*. Another which likewise fluctuates through a narrow compass on the two sides of the usual limit is in *Musca*, and has a period of 84 hours, more or less. I have also reason to believe that α, β, and ν *Octantis*, are all of them more or less variable.

"On the 17th October the Observatory is to be formally inaugurated, and I have good reason to believe that within a month from this day I shall be able to begin the work upon the Zones, which formed the real motive of my expedition hither.

"I have seen with some interest the various discussions in the *Monthly Notices* of the Royal Astronomical Society regarding the motion of η *Argûs* in its nebula, or rather regarding alleged changes of form in the nebula itself. Thus far I have only been able to use a portable telescope of about five inches aperture, placed upon the roof of my house. But such observations as I have been able to make, compared with the drawing in Sir J. Herschel's Cape Observations, have tended strongly to impress me with the conviction that the alleged change is altogether imaginary.

"How much I wish Maclear's observations had been accessible you may imagine."

Note on the Inferior Conjunction of Venus. By Capt. W. Noble.

On Sept. 26, I observed *Venus* $1^h 37^m$, after she had passed her inferior conjunction. The state of the atmosphere was too bad to admit of any attempt at micrometrical measurements: but I estimated that her illuminated crescent occupied a little more than a third of her circumference. My chief reason, however, for putting this observation on record is that I quite failed to see the dark body of the planet, which under analogous conditions has always been visible enough before in a constricted field. Doubtless the wretched state of the air may have had something to do with it; but is it possible that the bright background (whatever it may be) on which *Venus* must be projected varies in lustre?

Occultations of Stars by the Moon, observed at Forest Lodge, Maresfield. By Capt. W. Noble.

1871, September 24.

ϕ Capricorni.

The star disappeared instantaneously at the Moon's dark limb at $22^h 50^m 33^s.5$ L.S.T. $= 10^h 37^m 18^s.14$ L.M.T. Power 154 adjusted on the star. I did not observe the reappearance. The atmospheric undulation was tremendous.

1871, October 23.

τ^1 Aquarii.

The star disappeared instantaneously at the Moon's dark limb at $22^h 49^m 44^s$ L.S.T. $= 8^h 42^m 27^s.5$ L.M.T., and reappeared at her bright limb somewhere about $23^h 23^m 56$ L.S.T. $= 9^h 16^m 34^s$ L.M.T. It was, however, several seconds after this the star was caught at a little distance from the Moon's limb; but the motion of the Moon was, so to speak, so oblique to the parallel of the star's declination, and the definition was so awful from great atmospheric disturbance, that I put this down with much hesitation.

τ^2 Aquarii.

This star, a much more conspicuous one than the previous object, disappeared instantaneously at the Moon's dark limb at $23^h 57^m 27^s.8$ L.S.T. $= 9^h 50^m 0^s.2$ L.M.T.; and reappeared, pretty sharply, at the bright limb at $1^h 7^m 29^s.8$ L.S.T. $= 10^h 59^m 50^s.7$ L.M.T.

The Moon's limb was very mountainous and uneven where the star reappeared; and the bubbling and boiling were as bad as, or worse than, ever.

Power 255 adjusted on the stars respectively.

1871, *Nov.* 10.

Self-recording Transit Micrometer. By A. S. Herschel, B.A., F.R.A.S.

Supposing that, in the annexed figure, the motion of a vertical micrometer wire, *a b*, can be adjusted exactly to that of a star in the field of view of a transit instrument, the following automatic arrangement affords the means of chronographically recording the transit of the wire, or of the star, across any moderate number of imaginary fixed vertical wires, at any convenient distances from each other in the field of view. The principal object endeavoured to be realized in the contrivance is that it may be reversible, or equally effective in its action whether the motion of the wire is from right to left, as in observing a star below the pole, or, as in the case of a star's transit above the pole, when the wire moves in the opposite direction. This condition was, however, not the only object kept in view, and believed to be satisfactorily fulfilled in its construction. The principle successfully adopted by Sir Joseph Whitworth to measure extremely small intervals of length (not exceeding the millionth part of an inch),

$$+ \frac{[illegible]}{4096} m^{6} \ldots$$

$$+ \frac{62329307}{24576} m^7 (1+Q)^{-6}$$

(where writing $Q = 0$, and omitting the terms in m^4 and m^5 in the coefficient of $(1 + Q)^{-1}$, and the term in m^5 in the coefficient of $(1 + Q)^{-2}$, we have the original formula of p. 237). And

$$
\begin{aligned}
h = nt \Big\{ & \left[-\frac{45}{32} m^2 - \frac{1935}{512} m^3 \right] \frac{a^2}{a'^2} \\
+ & \left\{ \begin{aligned} & \left(-\frac{3}{4} + \frac{3}{2}\gamma^2 - \frac{3}{2}e^2 - \frac{9}{8}e'^2 - \frac{51}{8}\gamma^2 e^2 + \frac{9}{4}\gamma^2 e'^2 + \frac{21}{64}e^4 - \frac{9}{4}e^2 e'^2 - \frac{45}{32}e'^4 \right) m^2 \\ & \qquad + \left(\frac{171}{128}\gamma^2 - \frac{243}{128}e^2 \right) m^4 \\ & \qquad + \left(-\frac{387}{256}\gamma^2 + \frac{7785}{256}e^2 \right) m^5 \end{aligned} \right\} (1+Q)^{-1} \\
+ & \left\{ \begin{aligned} & \left(\frac{9}{32} - \frac{27}{16}e^2 - \frac{189}{32}\gamma^2 + \frac{23}{32}e'^2 + \frac{27}{16}\gamma^4 + \frac{567}{16}\gamma^2 e^2 - \frac{99}{16}\gamma^2 e'^2 - \frac{675}{256}e^4 - \frac{349}{16}e^2 e'^2 \right) m^3 \\ & \qquad + \left(-\frac{1539}{1024}\gamma^2 - \frac{15309}{2048}e^2 \right) m^5 \end{aligned} \right\} (1+Q)^{-2} \\
+ & \left(\frac{177}{128} - \frac{195}{64}\gamma^2 - \frac{699}{32}e^2 + \frac{2685}{256}e'^2 \right) m^4 (1+Q)^{-3} \\
+ & \left(\frac{10949}{2048} - \frac{6369}{512}\gamma^2 - \frac{133839}{1024}e^2 + \frac{75759}{1024}e'^2 \right) m^5 (1+Q)^{-4} \\
+ & \frac{467977}{24576} m^6 (1+Q)^{-5} \\
+ & \frac{26983045}{589824} m^7 (1+Q)^{-6}
\end{aligned}
$$

(where writing $Q = 0$, and omitting the terms in m^4 and m^5 in the coefficient of $(1 + Q)^{-1}$, and the term in m^5 in the coefficient of $(1 + Q)^{-2}$, we have the original formula of p. 238). We hence have

$$
\begin{aligned}
l &= nt\{A + (1+B)Q + CQ^2\}, \\
&= nt\{A + Q + BQ + CQ^2\}, \\
g &= nt\{A' + B'Q + C'Q^2\}, \\
h &= nt\{A'' + B''Q + C''Q^2\},
\end{aligned}
$$

where (omitting the terms in $\frac{a^2}{a'^2}$)

$$A = 1 + \left(-\frac{7}{4} + \frac{21}{2}\gamma^2 - \frac{3}{4}e^2 - \frac{21}{8}e'^2 + \frac{33}{4}\gamma^4 - \frac{39}{8}\gamma^2 e^2 + \frac{63}{4}\gamma^2 e'^2 - \frac{9}{8}e^2 e'^2 - \frac{109}{32}e'^4\right)m^2$$
$$+ \left(-\frac{225}{32} + \frac{81}{4}\gamma^2 - \frac{675}{64}e^2 - \frac{825}{32}e'^2 - \frac{243}{4}\gamma^4 + \frac{1863}{32}\gamma^2 e^2 + \frac{629}{8}\gamma^2 e'^2 + \frac{2025}{256}e^4 - \frac{105}{32}e'^4\right)m^3$$
$$+ \left(-\frac{3265}{128} + \frac{14577}{128}\gamma^2 - \frac{1833}{64}e^2 - \frac{48225}{256}\right)m^4$$
$$+ \left(-\frac{243925}{2048} + \frac{177333}{256}\gamma^2 - \frac{328065}{4096}e^2 - \frac{1502265}{1024}e'^2\right)m^5$$
$$+ \left(-\frac{12626759}{24576}\right)m^6$$
$$+ \left(-\frac{1365131021}{589824}\right)m^7.$$

$$B = \left(\frac{7}{4} - \frac{21}{2}\gamma^2 + \frac{3}{4}e^2 + \frac{21}{8}e'^2\right)m^2$$
$$+ \left(\frac{225}{16} - \frac{81}{2}\gamma^2 + \frac{675}{32}e^2 + \frac{825}{16}e'^2\right)m^3$$
$$+ \frac{9795}{128}m^4$$
$$+ \frac{243925}{512}m^5.$$

$$C = -\frac{7}{4}m^2$$
$$- \frac{675}{32}m^3.$$

$$A' = \left(\frac{3}{2} - \frac{15}{2}\gamma^2 + \frac{9}{8}e^2 + \frac{9}{4}e'^2 - \frac{45}{4}\gamma^4 + 15\gamma^2 e^2 - \frac{45}{4}\gamma^2 e'^2 - \frac{27}{16}e^4 + \frac{27}{16}e^2 e'^2 + \frac{45}{16}e'^4\right)m^2$$
$$+ \left(\frac{27}{4} - \frac{351}{16}\gamma^2 - \frac{297}{64}e^2 + \frac{401}{16}e'^2 + \frac{135}{2}\gamma^4 - \frac{1053}{32}\gamma^2 e^2 - \frac{1297}{16}\gamma^2 e'^2 + \frac{675}{256}e^4 - \frac{1079}{64}e^2 e'^2\right)m^3$$
$$+ \left(\frac{1995}{64} - \frac{16833}{128}\gamma^2 - \frac{19209}{512}e^2 + \frac{29535}{128}e'^2\right)m^4$$

$$+\left(\frac{17709}{128}-\frac{785575}{1024}\gamma^2-\frac{999051}{4096}e^2+\frac{883245}{512}e'^2\right)m$$

$$+\frac{2431349}{4096}m^6$$

$$+\frac{62329307}{24576}m^7.$$

$$B'=\left(-\frac{3}{2}+\frac{15}{2}\gamma^2-\frac{9}{8}e^2-\frac{9}{4}e'^2\right)m^2$$

$$+\left(-\frac{27}{2}+\frac{351}{8}\gamma^2+\frac{297}{32}e^2-\frac{401}{8}e'^2\right)m^3$$

$$-\frac{5985}{64}m^4$$

$$-\frac{17709}{32}m^5$$

$$C=\frac{3}{2}m^2$$

$$+\frac{81}{4}m^3.$$

$$A''=\left(-\frac{3}{4}+\frac{3}{2}\gamma^2-\frac{3}{2}e^2-\frac{9}{8}e'^2-\frac{51}{8}\gamma^2e^2+\frac{9}{4}\gamma^2e'^2+\frac{21}{64}e^4-\frac{9}{4}e^2e'^2-\frac{45}{32}e'^4\right)m^2$$

$$+\left(\frac{9}{32}-\frac{27}{16}\gamma^2-\frac{189}{32}e^2+\frac{23}{32}e'^2+\frac{27}{16}\gamma^4+\frac{567}{16}\gamma^2e^2-\frac{99}{16}\gamma^2e'^2-\frac{675}{256}e^4-\frac{349}{16}e^2e'^2\right)m^3$$

$$+\left(\frac{177}{128}-\frac{219}{128}\gamma^2-\frac{3039}{128}e^2+\frac{2685}{256}e'^2\right)m^4$$

$$+\left(\frac{10949}{2048}-\frac{15825}{1024}\gamma^2-\frac{220707}{2048}e^2+\frac{75759}{1024}e'^2\right)m^5$$

$$+\frac{467977}{24576}m^6$$

$$+\frac{26983045}{589824}m^7$$

instrument, conveying an impression, like that which is produced by a small tap of the finger on a transmitting key, to the revolving drum.

The wire having next advanced an interval equal to that contained between the bar $k\,k'$ and the projecting metallic part at k of the second bar $k\,k'$, a second current of the same kind will record the instant of contact between the first pair of bars, or the moment of the star's transit over the second imaginary vertical wire. After an interval corresponding to the distance between the second bar and the projecting part at l of the third bar, the moment of contact between this pair of bars will be similarly recorded, and the instant is that at which the star crossed the position of the central or third imaginary transit-wire. Advancing thus until the last pair of bars are made to touch each other, and the moment of the star's transit over the last imaginary wire $c_5\,c_5$ is thereupon recorded, the strongest extension-spring of the series, that acting upon the bar $n\,n'$ will then begin to be compressed, while the movable wire is carried in that direction to the furthest limit of the field of view. In this position, by an exactly reversed series of operations (when a star is observed above the pole, and the wire is driven by the screw in the opposite direction, following its motion), the instrument may be made to record automatically the times of passage of the star, or movable wire, over exactly the same positions of imaginary vertical wires in the field of view as those which it traversed when moving in the previous direction. It is at least important to observe that the *difference* from the same exact positions, thus oppositely obtained, may be expected to depend less upon want of mechanical accuracy in the construction (*durability of surfaces, rigidity of the bars and pins, and immobility of the bearing-points*, being supposed to be practically attainable), than upon the different sensibility of galvanic currents of ordinary strength (arising from the heat which they produce in their passage) to the exact moments of commencement, and interruption of mechanical contact between two surfaces of metal, which are first brought into contact by pressing them together, and are then separated by gradually withdrawing them from each other. With the use of feeble currents, and of a specially delicate recording-apparatus, like that recently exhibited to the British Association at Edinburgh, as introduced by Sir W. Thomson to record the very feeble currents despatched through long submarine cables; the condition of reversibility in its action being apparently fully satisfied, with the exception of a, perhaps needless, complexity, and consequent liability to derangement in its details, which a more ingenious mechanical disposition of its parts would not impossibly remove the remaining conditions of rapid precision in its signals, and of regularly recurring intervals between them appear to be, at least, provisionally, and, although less certainly, yet in the present form of a self-recording transit-micrometer, perhaps not quite inadequately fulfilled.

$$+\left(-\frac{1273925965}{589824}\right)m^7,$$

$$g = nt\left\{\left[\frac{45}{16}m^2+\frac{585}{32}m^3\right]\frac{a^2}{a'^2}\right.$$

$$+\left(\frac{3}{2}-\frac{15}{2}\gamma^2+\frac{9}{8}e^2+\frac{9}{4}e'^2-\frac{45}{4}\gamma^4+15\gamma^2e^2-\frac{45}{4}\gamma^2e'^2-\frac{27}{64}e^4+\frac{27}{16}e^2e'^2+\frac{45}{16}e'^4\right)m^2$$

$$+\left(\frac{27}{4}-\frac{351}{16}\gamma^2-\frac{297}{64}e^2+\frac{401}{16}e'^2+\frac{135}{2}\gamma^4-\frac{1053}{32}\gamma^2e^2-\frac{1297}{16}\gamma^2e'^2+\frac{675}{256}e^4-\frac{1079}{64}e^2e'^2\right)m^3$$

$$+\left(\frac{1899}{64}-\frac{15009}{128}\gamma^2-\frac{20649}{512}e^2-\frac{28959}{128}e'^2\right)m^4$$

$$+\left(\frac{15981}{128}-\frac{663621}{1024}\gamma^2-\frac{1152843}{4096}e^2+\frac{847213}{512}e'^2\right)m^5$$

$$+\frac{2103893}{4096}m^6$$

$$+\frac{52802843}{24576}m^7,$$

$$h = nt\left\{\left[-\frac{45}{32}m^2-\frac{1935}{512}m^3\right)\frac{a^2}{a'^2}\right.$$

$$+\left(-\frac{3}{4}+\frac{3}{2}\gamma^2-\frac{3}{2}e^2-\frac{9}{8}e'^2-\frac{51}{8}\gamma^2e^2+\frac{9}{4}\gamma^2e'^2+\frac{21}{64}e^4-\frac{9}{4}e^2\,{}'^2-\frac{45}{32}e'^4\right)m^2$$

$$+\left(\frac{9}{32}-\frac{27}{16}\gamma^2-\frac{189}{32}e^2+\frac{23}{33}e'^2+\frac{27}{16}\gamma^4+\frac{567}{16}\gamma^2e^2-\frac{99}{16}\gamma^2e'^2-\frac{675}{256}\,{}^4-\frac{349}{16}e^2e'^2\right)m^3$$

$$+\left(\frac{273}{128}-\frac{843}{128}\gamma^2-\frac{2739}{128}e^2+\frac{3261}{256}e'^2\right)m^4$$

$$+\left(\frac{9797}{2048}-\frac{7185}{1024}\gamma^2-\frac{165411}{2048}e^2+\frac{73423}{1024}e'^2\right)m^5$$

$$+\frac{199273}{24576}m^6$$

$$+\frac{6657733}{589824}m^7,$$

Mean Position for 1870.

R.A.	P.	Number and Comparison in R.A.	P.	Remarks.	Comparison Stars.	
h m s	° ′ ″					
17 54 15·64	65 6 12·1	5	5	Very small and very faint nucleus of 15th Mag.	33,103 Lat.	8
18 7 15·14	64 22 41·6	2	2	Scarcely visible. A vaporous aspect comprised between two very small stars.	33,555 Lat.	8
18 29 9·76	67 11 22·5	5	5	Extremely small and faint, round, gradually brilliant towards the middle.	34,322 Lat.	7½
18 29 32·50	59 22 47·8	5	5	Extremely small and faint.	896 W. xviii.	9
18 32 11·95	63 41 34·8	5	5	Nucleus of 14th Mag. surrounded by a very faint nebulosity.	993 W. xviii.	9
18 33 15·35	64 44 11·9	5	5	A small nucleus, very faint, surrounded by a tolerably extensive nebulosity, round, the edges badly defined.	34,510 Lat.	8½
18 36 6·39	53 45 12·3	5	5	Round, tolerably extended, excessively faint on the edges, a brilliant point.	3,224 Arg. Z. + 36°	9, 3
18 45 19·95	63 18 50·1	5	5	Excessively small and faint, round, edges badly defined, condensation in the centre.	3,360 Arg. Z. + 26°	9, 3
18 58 16·48	68 34 54·4	5	5	Excessively small, tolerably brilliant, brilliant points towards the centre.	22 W. xix.	8
19 6 5·05	59 39 59·7	5	5	Round, small, tolerably brilliant, more brilliant in the middle, seems resolvable.	35,989 Lat.	6½
20 32 8·73	95 25 32·1	5	5	Small, round, whitish, moderately brilliant, more brilliant towards the centre.	39,827 Lat.	6
21 56 44·03	79 25 41·1	5	5	Irregularly round, very small, extremely faint, scarcely observable, towards the centre a little less faint.	42,973 Lat.	7
22 5 34·36	64 46 35·1	5	5	Round, small, very faint, vaporous aspect.	96 W. xxii.	8, 9
22 31 7·30	52 7 30·7	3	3	Small, moderate brightness, more brilliant towards the centre, egg-shaped.	772 W. xxii.	8

Mean Position for 1870.

Comparison Stars.		R. A.	P.
		h m s	° ′ ″
29,273 Lat.	6	15 57 58·92	71 50 19·1
73 W. xvi.	9	16 5 27·19	79 47 36·9
2,821 Arg. Z. + 41°	9, 2	16 17 7·32	48 49 20·3
5,515 B. A. C.	7½	16 23 1·38	57 0 33·1
1,225 W. xvi.	9	16 39 16·47	49 50 14·0
5,625 B. A. C.	7½	16 14 20·83	87 31 21·3
2,649 Arg. Z. + 28°	9, 1	16 55 57·19	61 56 11·4
1,717 W. xvi.	9	16 56 33·25	66 52 38·5
283 W. xvii.	8	17 11 20·48	69 31 6·4
32,068 Lat.	8	17 29 46·81	82 53 54·4
1,080 W. xvii.	9	17 34 30·12	71 3 29·7
α Ophiuchus		17 28 53·95	77 20 35·6
3,330 Arg. Z. + 25°	9, 3	17 37 28·70	64 29 41·9

Mean Position for 1870.

Comparison Stars.		R.A. h m s	P. ° ′ ″
3,118 Arg. Z. + 31°	9, 5	17 48 37·31	58 32 45·3
1,601 W. xvii.	7, 8	17 50 42·25	71 39 8·3
33,103 Lat.	8	17 56 15·86	65 1 51·7
33,555 Lat.	8	18 7 47·69	64 23 37·9
34,322 Lat.	7½	18 26 0·28	67 6 19·8
896 W. xviii.	9	18 30 42·79	59 26 35·9
993 W. xviii.	9	18 33 39·29	63 41 25·0
34,510 Lat.	8½	18 30 27·23	64 44 6·8
3,224 Arg. Z. + 36°	9, 3	18 35 17·46	53 47 38·2
3,360 Arg. Z. + 26	9, 3	18 42 5·75	63 19 11·5
22 W. xix.	8	19 2 35·01	68 25 28·9
35,989 Lat.	6½	19 4 21 42	59 38 34·0
39,827 Lat.	6	20 32 13·92	95 22 59·3
42,973 Lat.	7	21 56 6·57	79 15 22·8
96 W. xxii.	8, 9	22 4 53·92	64 49 29·9
772 W. xxii.	8	22 33 42·78	51 55 40·1

Observations of Encke's Comet with the Transit Circle and the Great Equatoreal at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

1871.	G.M.T. d h m s	Observed R.A. h m s	Correction to Mr. Hind's Ephemeris. s	Observed N.P.D. ° ′ ″	Correction to Mr. Hind's Ephemeris. ″	Observer.
Nov.	8 6 28 43	21 38 42·5	+11·0	..	..	J.C.
	6 28 48	21 38 47·1	+15·6	56 54 27·0	+49·0	P.
	9 6 15 7	21 29 0·9	+18·8	57 48 7·0	−17·0	J.C.

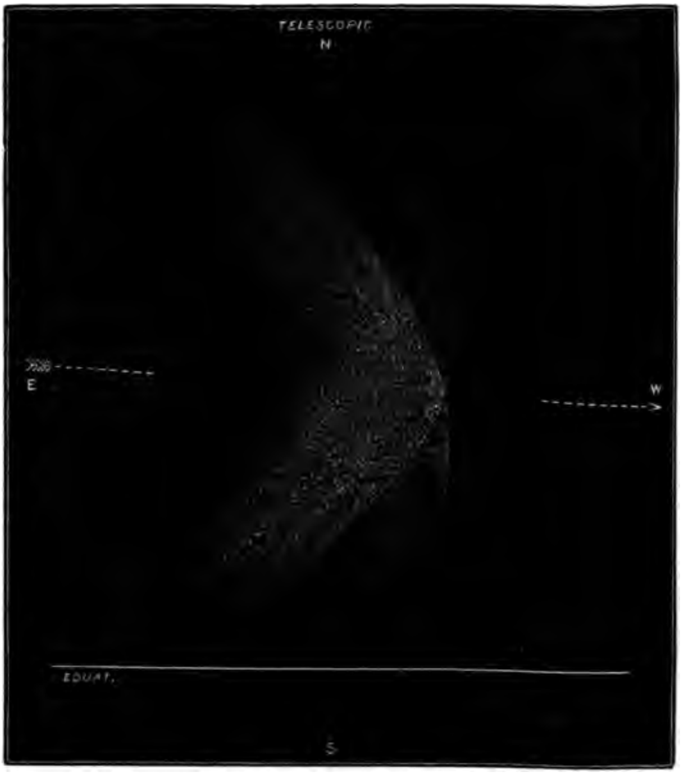
The observations were made by Mr. Carpenter and Mr. Potts.

Note by Mr. Carpenter:—"Nov. 9. The comet was large and faint, the diameter of the brightest portion equal to one interval of the wires used in eye-and-ear observations (about 240″). It was somewhat fan-shaped, with no nucleus; the part observed being that which I judged the brightest. The low-power eye-piece (about 50) was used."

In the first observation, on November 8, the transit was observed over two of the vertical wires, and in the second observation over one only. On November 9 the transit was observed over six wires.

Mr. Carpenter further writes: "I observed this comet last night, Nov. 9, with the Great Equatoreal (as well as with the Transit Circle). The appearance was the same as on the pre-

vious evening; but I was able to make out a considerable extension of the illumination beyond the bright fan-shaped condensation, but on one side (the spreading side) only. On the opposite side this diffused illumination appeared to be cut off nearly in a straight line immediately behind (following) the apex of the fan. (*See woodcut.*)



"The axis of the illuminated fan was inclined about 5° or 10° to the plane, the apparent west side (apex of the fan) dipping. The comet was easily seen in the finder of the Great Equatoreal."

1871, *Nov.* 10.

[The Astronomer Royal explained to the Meeting, by reference to a globe upon which the Sun and the Comet had been drawn in their proper positions, that the spreading side of the fan-shaped luminosity was turned almost directly towards the Sun.]

*Ephemeris of Encke's Comet.**

At Greenwich Midnight.

1871.		R.A. h m s	N.P.D. ° ′	Log. of Hor. Parallax.
Nov.	1	22 45 51	52 27·1	1·3943
	2	22 36 6	52 54·2	1·4023
	3	22 26 13	53 25·5	1·4098
	4	22 16 16	54 1·0	1·4169
	5	22 6 15	54 40·9	1·4234
	6	21 56 13	55 25·1	1·4293
	7	21 46 12	56 13·6	1·4346
	8	21 36 13	57 6·1	1·4394
	9	21 26 19	58 2·3	1·4436
	10	21 16 32	59 2·3	1·4471
	11	21 6 53	60 5·8	1·4501
	12	20 57 22	61 12·5	1·4524
	13	20 48 1	62 22·3	1·4541
	14	20 38 51	63 34·5	1·4552
	15	20 29 52	64 48·8	1·4556
	16	20 21 5	66 5·0	1·4555
	17	20 12 31	67 22·9	1·4548
	18	20 4 8	68 42·1	1·4535
	19	19 55 57	70 2·3	1·4516
	20	19 47 58	71 23·3	1·4491
	21	19 40 11	72 44·7	1·4460
	22	19 32 34	74 6·6	1·4424
	23	19 25 9	75 28·2	1·4383
	24	19 17 54	76 49·7	1·4337
	25	19 10 48	78 10·9	1·4286
	26	19 3 52	79 31·7	1·4236
	27	18 57 5	80 51·9	1·4169
	28	18 50 26	82 11·4	1·4103
	29	18 43 56	83 29·9	1·4032
	30	18 37 35	84 47·5	1·3957
Dec.	1	18 31 21	86 4·3	1·3877
	2	18 25 14	87 20·1	1·3793
	3	18 19 15	88 34·8	1·3704
	4	18 13 24	89 48·5	1·3611
	5	18 7 41	91 1·1	1·3514

The perihelion passage is assumed, 1871, Dec. 28·77375,

* This Ephemeris and the following one of Tuttle's Comet, both communicated by Mr. Hind, have been issued as circulars.—Ed.

G.M.T., as indicated by an observation at Mr. Bishop's Observatory, on October 10.

The Sun's Equat. Hor. Parallax = 8″·91.

J. R. Hind.

Twickenham, 1871, *Oct.* 30.

Ephemeris of Tuttle's Comet.

At Greenwich Midnight.

1871.	R.A. h m s	N.P.D. ° ′	Log. Δ.
Nov. 1	9 56 52	66 16·9	
2	9 59 4	67 39·5	9·9097
3	10 1 15	69 4·0	
4	10 3 24	70 30·5	9·8982
5	10 5 53	71 59·4	
6	10 7 41	73 30·7	9·8873
7	10 9 49	5 4·1	
8	10 11 57	76 39·7	9·8771
9	10 14 5	78 17·4	
10	10 16 13	79 57·1	9·8677
11	10 18 21	81 38·8	
12	10 20 29	83 22·5	9·8593
13	10 22 37	85 7·9	
14	10 24 46	86 55·0	9·8521
15	10 26 55	88 43·7	
16	10 29 5	90 33·8	9·8462
17	10 31 15	92 25·0	
18	10 33 26	94 17·4	9·8417
19	10 35 37	96 10·7	
20	10 37 50	98 4·7	9·8387
21	10 40 4	99 59·2	
22	10 42 19	101 53·8	9·8373
23	10 44 35	103 48·5	
24	10 46 53	105 42·9	9·8375
25	10 49 12	107 36·9	
26	10 51 33	109 30·3	9·8393
27	10 53 56	111 22·8	
28	10 56 21	113 14·3	9·8426
29	10 58 48	115 4·7	
30	11 1 17	116 53 6	9·8474

The ephemeris is calculated from Dr. Tischler's elements, but with perihelion passage corrected to December 1, 7974, G.M.T., by observations at Paris and Hamburg.

J. R. Hind.

Queries relative to Double Stars. By Sir J. W. F. Herschel, Bart.

The Committee appointed by the Council of Royal Astronomical Society to examine and report upon the valuable collection of papers bequeathed to the Society by the late Sir J. F. W. Herschel, entitled "General History of Double Stars," have requested that the following list of queries collected by him in the course of his laborious researches should be printed in the *Monthly Notices*, as an invitation to Astronomers furnished with good libraries and good telescopes to assist in the solution of the doubts respecting the stars contained in the list.

The list is given exactly as Sir John left it, without any attempt to diminish the number of doubtful cases by reference to Struve's *Positiones Mediæ*, or other works.

Star's Name.	R.A. h	m	s	N.P.D. °	′	″	
A 2021	1	0	37	109	31	40	Is it really close double?—"Violent suspicion."
Σ 168	1	40	17	20	47	30	Not seen double by h.
Σ 211	1	55	54	96	14	50	No such star found in, or near place by h.
Σ {343 347}	2	{55 56	23 27	6 6	35 35	32 2}	Are these the same double star? Σ is confused, and h disagrees.
Σ 407	3	21	56	101	43	4	? Is the minute of R.A. 19 or 21? Σ's two catalogues differ.
Σ' 417} Σ 518}	4	{7 7	27 48	97	{55 54	22 23}	40 Eridani. Confusion exists. Wanted an exact diagram description, and measures of A, B, C; and ? of a 4th star, D.
Σ 609	4	41	59	89	8	59	? Whether Struve's place, as here given from "Positiones Mediæ," or that of my 7th Catalogue 4h 43m 4s, 89° 3′ 17″ be right. Positions and distances agree. Or are they really two different double stars? *My place* is corroborated in 5th Catalogue, h 2240 *bis*, making the R.A. 43m.
Σ 662	5	5	39	94} 64}	15	54	In Σ "Pos. Mediæ," the P.D. is 94° 15′ 54″; but in the Dorpat Catalogue and also in the "Mensuræ Micrometricæ," the degree of P.D. is 64°. The declination given are Dorp. + 25° 45′; "Mens. Microm." + 25° 45′; "Pos. Med." − 4° 15′ 54″.
h 5465	5	39	18	78	4	3	An excessively minute *comes* 12″ distant suspected. ? if verified.
Σ 923	6	{22 42	15 15	30	25	8	? Which is the right minute of R.A.? Σ leaves it doubtful, and I have no observation of it.
Σ 1006	6	51	22	{26 27	59 10	18 18}	Which is the right P.D.? In the Dorpat Catalogue the declination is + 63° 1′; in the "Mens. Microm." + 62° 50′. Which is right?
Σ'(989) *g.c.* σ 295	{8 8	14 14	22 25	92 92	25 23	7 51}	Set down as "very near" σ 295 (Catalogi Prioris.) Are these really two different double stars? If not, which is right?
Σ 1259	8	32	7	51	5	29	In Dorpat Catalogue 8h 31m·9, Decl. + 38° 56′. In "Mens. Microm." 8h 36m, Decl. + 39° 5′. Query which is right. h has no observation.
δ Cancri	8	35	1	71	13	39	h 457 makes Position 160°, Distance 25″; Lassell, "Astr. Nach." No. 858, states it as Position 121°·12, Distance 45″. What is it really?

Star's Name.	R.A. h	m	s	N.P.D. °	′	″	
δ Cancri	12	29	–	75	17		Close to a star of 9th Mag. is a small, well-defined body, *which may be* a close double star. *sp* is a very faint nebula (no doubt III. 602, or Neb. No. 3113 of my general Catalogue.) ? Is it there still? Was it a planet?
κ Virginis	14	3	50	99	28	42	σ 451 (Catalogi Prioris.) Is it really a double star?
ω Boötis	14	54	40	64	18	58	σ 469. Is it really double?
h 1271	15	15	18	107	59	48	Are these identical; and if so, which P.D. (differing 1°) is the right? The measures agree.
h 4768	15	15	18	109	0	43	
h 256	15	32	2	71	40	0	Not found subsequently; but ? if properly looked for. Does it exist? Measures 5° *sf.*; distance 2″.
Σ 2012	16	0	49	97	48	13	Is this really identical with B.A.C. 5379?
h 584	16	7 / 13	58	50	20	45	R.A. between 7^m and 13^m. ? The right minute.
Σ′ (1814)	16	16	48	88	23	2	"Loco 2041" Are these really two double stars? And if so, to which does the description, Position 4°·40, Distance 3″·060, belong?
Σ 2041	16	16	48	88	21	35	
Mayer	17	1	29	114	2	43	Is there any double star in this place?
Σ 2190	17	29 ±		68	53		The measures of this star are very conflicting. Σ (1829) makes Position 33°·22. h (1830), by a sweep, 18°·4 (plainly written in original, and diagram agreeing.) Mädler (1843), 33°·67; and Dembrowski (1863), 23°·00. Which is right?
H 4599	18	57	0	58	32	41	Query as to its existence in this place, or very near it.
Σ 2545	19	27	32	100	31	57	Smyth described this as "closely following" a star R.A. $19^h\,27^m\,28^s$, and the R.A. $19^h\,27^m\,32^s$ given by him differs 2^m from Σ. Which is right?
	19	29	23				
Σ′ (2360)	19	40	4	57	31	25	Are these not identical; the P.D. applied to H h 640 being 11′ wrong?
H 4640	19	39	54	57	19	48	
Σ 2634	20	1	50	73	41	46	Smyth identifies his No. 733 (Cycle) with this star, and with H h 668; but his P.D. 73°33′54″ (for 1830) differs 8′. Are these two double stars?
σ 680	20	22	53	79	23	3	Are these the same double star? Σ 2690 is identified with Dawes (1); h (269); H h 691; and O.Σ 407.
Σ 2690	20	23	6	79	18	22	
h 2994	20	36	18	112	7	33	17 Capricorni. Requires verification as a Position 338°·7, Distance 20″. A *very* minute companion.
h 1619 / 1620	21	4	22	76 / 77	9 / 9	37 / 35	Are not these the same; but 1° P.D. mistaken in one or other? Which is right? 1620 is described as *quadruple*.
h 5517	21	14	54	103	36	6	18 *Aquarii*. Query a very minute companion preceding, at 13″ distance.
Σ 2792	21	15	42	61	46	6	Query if not the same, with 1° error in P.D. in one or other; and, if so, which is right?
h 1638	21	15	43	62	46	43	
h 935	21	19	20	56 / 57	28	26	Two observations differ by 1° P.D. Query which is right?
Σ 2827	21	38	54	28	9	54	"Loco Σ 2827." The latter star seems to be only the former, with its place better determined. Σ 2827 is a really existing double star = h 1690 = Σ 2827 of the "Mens. Microm." Are those really two double stars very near each other? h 1690's place is $21^h\,39^m\,32^s$, N.P.D. 27° 11′ 1″.
Σ′ (2628)	21	39	5	28	8	39	

Star's Name.	R.A. h	m	s	N.P.D. °	′	″	
h 1761	22	16	24	16	0	38	Oblong. Requires re-examination.
h 5529	22	28	57	95	6	4	*κ Aquarii.* An excessively minute companion strongly suspected.
h 3158	22	53	15	20	8	37	Excessively close. Requires verification. Position 45°.
{ Sh 358 / Σ 690 }	23	{ 46 / 49	20 / 22 }	59	{ 6 / 11	20 / 40 }	? If two distinct double stars. If only one, ? its place.

Note on a Pair of Differential Equations in the Lunar Theory.
By Prof. Cayley.

The equations

$$\frac{d}{dt}\frac{d\rho}{dt} - \rho\left(\frac{dv}{dt}\right)^2 + \frac{1}{\rho^2} = k m^2 \rho \left\{\frac{1}{2} + \frac{3}{2}\cos(2v - 2mt)\right\},$$

$$\frac{d}{dt}\,\rho^2\frac{dv}{dt} \qquad\qquad = j m^2 \rho^2 \left\{\qquad \frac{3}{2}\sin(2v - 2mt)\right\},$$

taking therein $j = k = 1$ in effect present themselves in the Lunar Theory, and particular integrals in series have been obtained, the development being carried to a great extent; but I give the results only as far as m^4, viz., writing

$$t - mt = D,$$

we have

$$v = t + \left(\frac{11}{8}m^2 + \frac{59}{12}m^3 + \frac{893}{72}m^4\right)\sin 2D$$
$$+ \frac{201}{256}m^4 \sin 4D,$$

$$1 = 1 + \frac{1}{6}m^2 \qquad \frac{179}{288}m^4$$
$$+ \left(\quad m + \frac{19}{6}m^3 + \frac{131}{18}m^4\right)\cos 2D$$
$$+ \frac{7}{8}m^4 \cos 4D,$$

In the Lunar Theory j and k are properly each $= \frac{1}{1+\frac{E}{m'}}$ (E the mass of the Earth, m' that of the Sun), but they are taken to be $= 1$; the numerical difference is inappreciable; but there would be a considerable theoretical advantage in retaining in the equations the coefficients j, k: in fact, the developments could then be arranged according to the powers of k, that is according to the powers of the disturbing force; whereas, when k is taken $= 1$, we have only a development in powers of m, and since m also presents itself through the coefficient $2 - 2m$ of t in $2v - 2mt$, terms which are really of different orders in regard to the disturbing force, are united together into a single term: in fact, instead of a term of the form $(Ak + Bk^2 + \&c.)\, m^p$, where A, B, are numerical,

we have the term $(A + B + \ldots)\, m^p$, where of course $A + B \ldots$ is given as a single numerical coefficient. There is no equal advantage in retaining the two coefficients k, j, as this only serves to show how a term arises from the central and tangential forces respectively; thus retaining these coefficients, the integrals as far as m^2 are

$$v = t + \left(\frac{1}{2}k + \frac{7}{8}j\right) m^2 \sin 2\mathrm{D},$$

$$\frac{1}{\varrho} = 1 + \frac{1}{6}m^2 k + \left(\frac{1}{2}k + \frac{1}{2}j\right) m^2 \cos 2\mathrm{D},$$

agreeing with the former result when $k = j = 1$; but there is, nevertheless, some interest in retaining the two coefficients. I hope to develope the results somewhat further, and to communicate them to the Society.

Observations of Comets from B.C. 611 *to* A.D. 1640, *extracted from the Chinese Annals, translated, with Introductory Remarks, and an Appendix comprising Tables for reducing Chinese Time to European Reckoning, and a Chinese Celestial Atlas.* By John Williams, F.S.A., Assistant-Secretary of the Royal Astronomical Society. London: Printed for the Author, by Strangeways and Walden. 4to, 1871.

The Author was, by a question put to him by Mr. Hind, led to examine Biot's Catalogue of Comets observed in China; and he quickly found that, although very accurate in its details, it was by no means so complete as could be wished, many comets being recorded in the "Encyclopædia" of Ma Twan Lin, and in the great historical work called the "She Ke," which are not noticed by Biot. It therefore occurred to him that a catalogue comprising the whole of the observations of the comets contained in the two Chinese works just referred to, translated from the original, and arranged chronologically, with an explanation of all the particulars connected with them, might be of service to astronomers, particularly to those engaged in cometary researches. The observations thus brought together amount to 373.

The introductory remarks comprise many interesting particulars relating to Chinese astronomy, and also instructions for the use of the chronological tables contained in the appendix, giving the succession of the dynasties and emperors from the earliest period to the present time; and of the other tables for finding the months, or moons, and days. The Appendix includes also a complete Chinese celestial atlas from an original work, so that the names and relative positions of the asterisms and stars can be readily found.

As this work has been privately printed by the Author, it forms no part of the publications of the Society. It can be obtained (at a reduced price to Fellows) by application to Mr. Williams at the rooms of the Society.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXII. *December* 8, 1871. No. 2.

WILLIAM LASSELL, Esq., F.R.S., President, in the Chair.

Duncan Darrock, Esq , 5 Beaufort Gardens, and
Edwin George Monk, Esq., York,

were balloted for and duly elected Fellows of the Society.

On Telescopic Observations of the Phenomena seen in contiguity with the Moon's Limb during Eclipses of the Sun, and the Results which have been therefrom deduced. By Professor Grant.

The application of the spectroscope to observations of the envelope of red matter encompassing the solar photosphere, an envelope which has been named *the chromosphere*, has given rise to occasional allusions to the anterior telescopic observations of the same phenomenon, as revealed during eclipses of the Sun, and to the results hence deduced. I beg leave to submit to the Society a few remarks on this subject.

Admitting the existence of such an envelope and its thickness to be inconsiderable, the question arises, under what appearance should it present itself during eclipses of the Sun.

Since the apparent diameter of the Moon sometimes exceeds, sometimes equals, and sometimes falls short of, the apparent diameter of the Sun, it is obviously possible that the existence of the envelope may be occasionally revealed by the phenomenon of a circle of red light surrounding the Moon's disk during slightly total eclipses of the Sun. No instance, however, of such an occurrence has been recorded in the annals of telescopic observations of solar eclipses.*

* Secchi supposes the envelope encompassing the solar photosphere to have a thickness included between 10″ and 15″. Let us assume its mean thickness to be 12″·5. Now, since the apparent diameter of the Moon may be equal to, or greater or less, than the apparent diameter of the Sun, it is quite conceivable that a total eclipse of the Sun may occur during which the envelope may be seen

In solar eclipses generally it is plain that the existence of such an envelope may be *partially* revealed by the appearance of an arc of red light at the part of the Moon's limb where it just covers or slightly overlaps the Sun's disk. This partial covering or slight overlapping of the Sun's disk may occur — 1°, during a total eclipse; 2°, during a nearly total eclipse; 3°, during an annular eclipse; 4°, during a nearly annular eclipse. Let us consider each of these cases separately.

1. *Total Eclipses.*—If the observer be situated in the line of central totality the Moon would just cover the solar disk at the instants of immersion and emersion, and there should be visible a corresponding arc of red light contiguous to the Moon's limb, namely, first at the eastern and then at the western limb, having its centre in each case coincident with the point of interior contact of the solar and lunar disks. If the observer proceeds northwards the Moon's centre will be depressed by parallax, and the points of interior contact will gradually recede upon the upper limb, approaching each other and finally merging together at the apex when the observer has reached the northern limit of totality. Similarly, if the observer travel southwards, the points of interior contact will gradually shift upon the lower limb, approaching each other, and finally merging together at the apex when the observer has reached the southern limit of totality.

2. *Nearly Total Eclipses.*—If the observer be situated a little beyond the northern or southern line of totality, the arc of red light may be visible at the part of the upper or lower limb of the Moon where it slightly overlaps the solar disk.

3. *Annular Eclipses.*—The results as regards the shifting of the points of interior contact are here the same as in the case of total eclipses, except that, as the observer proceeds *northwards*, the points of interior contact approach each other on the *lower* limb instead of the *upper*, finally merging at the apex, while a result, the reverse of this, occurs when he travels towards the *southern* limit of the annular phase.

4. *Nearly Annular Eclipses.*—If the observer be situated a little beyond the northern or southern limit of the annular phase, the arc of red light (modified perhaps in aspect by the presence of Sunlight) may be expected to be seen at the part of the Moon's limb where it slightly overlaps the solar disk.

It is manifest then that in total eclipses of the Sun the interior contacts of the Sun and Moon, and the consequent slight over-

in the form of a red circle encompassing the Moon's limb. Such a condition may be realised on the 30th of November, 1872, when, according to the *Nautical Almanac*, there will be a total eclipse of the Sun, the maximum duration of which on the Earth's surface is 47 seconds. In Latitude 35° 6′ S. and 139° 25′ East Longitude the duration on the central line is stated to be 26 seconds, whence it may be inferred that the envelope would be visible, in the form of a complete circle, during about twenty seconds of time. Mr. Hind has remarked that if we assume the tabular values of the Sun and Moon to be exact, this eclipse will be *annular* near the times of beginning and ending on the Earth' *surface.*

lapping of the Moon beyond the solar disk, may occur at any part of the Moon's limb, being dependent in this respect on the position of the observer, with respect to the line of central totality. Of course the more nearly the apparent diameters of the Sun and Moon approach to an equality, the narrower will be the limits of totality, and the more rapidly will the points of contact shift upon the Moon's limb for any given change in the position of the observer relative to the central line of totality. On the occasion of the total eclipse of 1842 (which cannot be considered a *large* eclipse) the Moon's shadow passed over some of the most densely populated and easily accessible regions of Southern Europe, and experienced observers were stationed at a great number of places ranging between the two extreme limits of the shadow.

The remarks which I have made with respect to the shifting of the points of interior contact in the case of total eclipses of the Sun, are equally applicable to annular eclipses. As regards nearly total eclipses or nearly annular eclipses, the slight overlapping of the Moon will obviously occur either at the upper or the lower limb.

I would remark, further, that in total eclipses of the Sun observations made at the two extreme limits of totality are especially favourable for observing the arc of red light, since the Moon glides tangentially over the arc, and lingers upon it, whereas in the case of the observer being situated in the centre of the shadow, the Moon advances in a direction which is normal with respect to the arc (at its middle point), and, consequently, passes more rapidly over it. The observations made in France at the two opposite limits of totality during the eclipse of 1842 strikingly illustrate this circumstance.

A similar remark is obviously applicable to annular eclipses.

It was thus, by examining all the recorded observations of solar eclipses available to me from the beginning of the seventeenth century down to the annular eclipse of 1847, analysing and arranging them according as they were total, annular, nearly total, or nearly annular, and confronting the results with each other, that I was led, in the winter of 1850–51, to announce the existence of an envelope of matter superposed upon the solar photosphere.* I shall now quote a few of the observations included in this inquiry.

Total Eclipses of the Sun.

Halley, who observed the eclipse of 1715 at London, remarked that two or three seconds before the emersion, a long and very narrow streak of a dusky but strong red light seemed to colour the dark edge of the Moon on the western side where the Sun was just coming out.

I shall cite four observations of the phenomenon made during

* *History of Physical Astronomy*, pp. 358–411. This work was published in separate parts or numbers, in connexion with a republication of the works of the Library of Useful Knowledge. The first number appeared in September 1848, and the work was finally published complete in March 1852.

the eclipse of 1842, those of Schumacher and Radman made near the central line of totality, and those of Bérard and Guérin at the opposite limits. The observations of Schumacher were made at Vienna, which was situated towards *the north* of the central line of totality. A mean of the results derived from the tables of Burckhardt and Damoiseau made the distance of the upper limbs of the Sun and Moon to be 13″·66. Schumacher states that a little before the emersion of the Sun there appeared near the part of the Moon's disk from behind which the first ray of light was about to issue, *a narrow streak of rose-coloured red light*, which extended perhaps over an arc of 70° or 80° along the Moon's limb, and which disappeared with the luminous ring and the rose-coloured mountains as soon as the first ray of the Sun darted forth. The observations of Radman were made at Padua, which was situated towards *the south* of the line of central totality. These observations have an exceptional value in so far as the question under consideration is concerned, Professor Belli, of the University of Padua, having assigned to Radman the task of specially noting the phenomena visible at the Moon's limb immediately after the total immersion of the Sun, and immediately previous to its emersion. Radman states that no sooner was the Sun totally covered than there appeared a very slender streak of a reddish light surrounding somewhat less than half the circumference of the Moon disposed equally on each side of the place where the last ray of the Sun was extinguished. He adds that it soon vanished, but that it reappeared immediately previous to the emersion of the Sun. The observations of Bérard, who was a Captain in the French Marine and a Corresponding Member of the French Institute, were made at Toulon. Any one may convince himself by referring to the track of the Moon's shadow, as laid down in the *Connaissance des Temps* for 1842, that Toulon was situated almost exactly upon the *southern* line of totality. The observation of Captain Bérard is on this ground so important, that I will give his words in the original. He says,—

"Pendant tout le temps de l'éclipse totale on vit au de là du bord de la Lune, près de la region où le Soleil émergea, une bande rouge très mince dentelée irregulièrement ou comme sillonné ça et là de crevasses."

We have here a precise description of the *sierra* so well known to observers of subsequent eclipses. The visibility of the phenomenon during the whole time of obscuration is in perfect accordance with the position of the place of observation at one of the extreme limits of totality.

At Visan, which was very nearly upon the *northern* line of totality, Guérin, a French observer, perceived immediately after the immersion of the Sun, seven or eight very distinct indentations which might have been taken for so many stars of different magnitudes. They were redder, but less brilliant, than *Mars*. If the whole periphery of the Moon's disk had been bordered with similar luminous points it would have presented the appearance of

a box of ebony garnished with rubies. The phenomenon continued visible during the whole time of totality.*

Annular Eclipses.

Maclaurin, who observed the annular eclipse of 1737, states that a little before the formation of the ring, a remarkable point or speck of pale light appeared near the middle of the part of the Moon's limb that was not yet come upon the disk of the Sun, and that a gleam of light more faint than this light seemed to be extended from it to each horn. This phenomenon was seen about fifteen seconds previous to the projection of the whole body of the Moon upon the solar disk. Lord Aberdour, who observed the same eclipse, remarked that a narrow streak of a dusky red light coloured the dark edge of the Moon immediately before the ring was completed, and after it was dissolved. Henderson, who observed the eclipse of 1836 at Edinburgh, states that previous to the formation of the annulus, when the cusps were about 30° or 40° apart, an arc of faint reddish light was seen extending between them. The appearance lasted several seconds.† Dr. Forster, who observed the eclipse of 1847 at Bruges, mentions that there was visible a very remarkable luminous arc or ridge of light, differently coloured from the rest of the Sun, extending along and immediately on the limb of the Moon between the cusps. From the brief notice of this observation it is not very clear whether it related to an annular or a nearly annular eclipse. It would follow from an inspecfion of the track of the Moon's penumbra, as laid down in the *Connaissance des Temps* for 1847, that Bruges was situated a very little distance beyond the northern limit of annularity.

Nearly Annular Eclipses.

Short, who observed the eclipse of 1748, at Aberdour Castle, where it was very nearly annular (a little beyond the southern limit), states that when the interval between the Moon's cusps did not exceed one-seventh of the Moon's circumference, a brown light was plainly perceived both by Lord Morton‡ and himself to extend from each cusp about one-third of the whole distance between them. He remarked also at the extremity of this brown light (which came from the western cusp) a larger quantity of light than elsewhere. The illustrious Bessel thus describes the appearance presented during the eclipse of 1836, May 15, as observed at the Königsberg Observatory, which was situated a very short distance beyond the northern limit of the annular phase:—"About twenty-five seconds before the nearest approach of the centres of the two bodies, I perceived near the extremity of the upper cusp a luminous point which, without having the brightness of the Sun, was very dis-

* Guérin adds that the luminous points extended over an arc of about the sixth part of the Moon's circumference.

† Edinburgh was situated about midway between the central and northern line of the annular phase. The first annular contact took place at 167° from the vertex towards the west.

‡ A good amateur observer, the same person who, under the name of Lord Aberdour, observed the annular eclipse of 1737, vide suprà.

tinctly visible with the powerful heliometer. As the cusps were then approaching each other, I hoped that the annulus was about to form, but this did not happen. With respect to the point which I have just alluded to, it became more luminous. Other similar points appeared which soon united together, and rendered visible all the part of the Moon's limb included between the extremities of the cusps."* Mr. Twining, an American, observed the eclipse of 1838 at New York. This eclipse was annular at Philadelphia, but failed very slightly in being so at the place where Mr. Twining observed it. He states that several minutes before the nearest completion of the ring, the fine cusps of the Sun's unobscured crescent were prolonged by a hair-breadth line of brightness totally diverse in colour and intensity from the Sun's disk. As the cusps approached the lines or threads of light shot round the Moon's edge between them rapidly, till at a certain time they met and joined in one, thus uniting the cusps.

The visibility of the phenomenon during sunshine, as thus exhibited in the case of annular and nearly annular eclipses, is fully confirmed by the observations of the annular eclipse of May 23, 1867, made in Dalmatia, at Barsecienne, a place situated on the northern limit of the annular phase. On that occasion Herr Kiha, an officer of the Austrian naval service, perceived a prominence near the extremity of the upper cusp 14·4 minutes before the formation of the ring, and was able to follow it during a further interval of 14·7 minutes.† The colour (*braünlich gelb*) which he ascribes to the object accords perfectly with the speck of pale light seen by Maclaurin during the annular eclipse of 1737, with the accumulation of brown light at a point intermediate between the cusps noticed by Short during the nearly annular eclipse of 1748, and furthermore with the observations of Bessel and Henderson in 1836, of Twining in 1838, and Forster in 1847.

In every instance, except one or two, it is expressly stated that the arc of red light was seen at the region of immersion or emersion,—in other words, at the region where the Moon's limb just covered, or slightly overlapped, the Sun's disk. In no instance is it stated to have been observed at any other part of the disk. In annular or nearly annular eclipses, the break observed in some instances in the arc extending from the cusps, is accounted for by the somewhat too great overlapping of the Moon, which would be most effective at the point midway between the cusps.

While engaged in this inquiry I endeavoured to show that the phenomena observed during solar eclipses, whether corona, streaks or patches of light, or prominences great or small, *are*

* Secchi, in his recently published work *Le Soleil*, p. 213, remarks: "Bessel, observant une eclipse annulaire à Königsberg, avait vû le bord de la lune environné d'un circle rouge." This statement must not be accepted in a literal sense. The eclipse was *not* annular at Königsberg, although very nearly so, and most assuredly Bessel did not see the margin of the Moon surrounded by a red *circle*. The account of the observation which I have given is a translation of Bessel's own description of the phenomenon (*Ast. Nach.* vol. xiv. p. 115).

† ... *Nach.* No. 1647, p. 228.

all solar phenomena. While the results of observation were either positively in favour of the solar aspect of the case or adverse to the lunar aspect, there did not appear to exist a shadow of a proof in support of the lunar origin of the phenomenon.

Seeing, then, that when a great number of solar eclipses, observed indifferently with respect to the limiting lines of the Moon's shadow or penumbra, are taken into consideration, the slight overlapping of the Moon takes place at all parts of the solar disk, and that wherever this overlapping really does occur, there is visible at the Moon's limb an arc or band of red light, the existence of such a margin of light becomes thus inductively traceable round the entire contour of the solar disk.

It was under the influence of a conviction forced upon me by these considerations that in the winter of 1850–51 I made the following announcement:—

"According to the usual theory of the physical constitution of the Sun, that body consists of an opaque nucleus, surrounded by an atmosphere in which are suspended, at different elevations, two enveloping substances of dissimilar physical properties, the lower envelope being imperfectly luminous, but capable, in a high degree, of reflecting light, while, on the other hand, the upper envelope forms a resplendent canopy of clouds, which are luminous in themselves, and constitute the source of the light diffused in every direction by the Sun. The observations of solar eclipses would seem to indicate that, above the luminiferous envelope, there exists *a stratum of nebulous matter* which is visible only by means of reflected light. Various interesting questions present themselves for solution in connexion with the admitted existence of such a stratum. In the first place, does *this third envelope* exercise an influence in the production of any of the other phenomena which have been disclosed by observations on the physical constitution of the Sun? M. Arago has very ingeniously suggested that the solar spots which exhibit the aspect of a penumbra without a nucleus, may arise from the superposition of masses of this nebulous matter above the luminiferous envelope. Secondly, the question arises, does any relation subsist between this non-luminous substance, which floats in the upper regions of the solar atmosphere, and either of the other two envelopes?

"Sir John Herschel, in his ingenious theory of the physical origin of the solar spots, supposes a perpetual circulation to be kept up in the solar atmosphere, analogous to that which, in the case of the terrestrial atmosphere, produces the phenomenon of the trade-winds. Now, is it not reasonable enough to suppose that such a circulation will have the effect of continually agitating the non-luminous matter constituting the envelope nearest the solid nucleus of the Sun, and throwing up masses of it above the luminiferous surface?

"This view of the subject, while it carries with it considerable probability, obviates the necessity of introducing into the theory of the physical constitution of the Sun, the idea of a third

ivelope independent of the two others. Future observations rosecuted more especially with reference to the positions of the solar spots during the occurrence of eclipses of the Sun, can alone be expected to throw light upon this interesting question." (*History of Physical Astronomy*, page 400.)

The passage which I have here quoted contains the announcement of a fact, and an attempt at an explanation of its physical cause. As regards *the fact*, I consider it to have been established by the observations on which it is founded. With respect to *the explanation*, it is a mere surmise which may or may not contain elements of truth.

In referring to this solution which I gave of the question of the band of red light seen during solar eclipses, I am far from maintaining that it is as perfect as it might be. The combination of the valuable results of telescopic observations of solar eclipses made subsequently to the annular eclipse of 1847, with the previously existing stock of results, would unquestionably improve the solution, and a better still might be obtained by a systematic placing of observers in determinate positions with respect to the limits of the track upon the Earth's surface of the Moon's shadow or penumbra; but now observations on a new principle are available which supersede the necessity of any such attempts.

The interest attached to the envelope of red matter encompassing the solar photosphere, and the importance with which it has been recently invested, are almost wholly due to the brilliant results which have been obtained by the application of the spectroscope to observations of the Sun. Still it appears to me to be desirable, in the interest of truth, that the series of carefully executed telescopic observations of solar eclipses anterior to the year 1848 should have accorded to them their due weight, and that their applicability to a legitimate explanation of some of the phenomena seen on such occasions should be fairly stated.

The Observatory,
Glasgow, November 1871.

In connexion with the foregoing remarks I append an extract from an account of my own observations of the total eclipse of the Sun of July 18, 1860, which I communicated to the Astronomer Royal in the autumn of 1860. The observations were made from a position (about twenty miles north of the line of central totality) on the Sierra de Tolonio, a lofty mountain range, a little to the south of Vitoria, overlooking the valley of the Ebro.

"At a distance of 60° to the right of the vertex, I perceived a third prominence. When first seen it resembled a small cone attached to the Moon's limb. As the Moon advanced over the Sun's disk, it gradually increased in dimensions until it finally attained an altitude of about 1½′. In colour and general aspect it bore a close resemblance to the large prominence near the vertex. "I was engaged in examining the last-mentioned promin-

ence I perceived several other very small peaks which had just come into view, and a few seconds afterwards the motion of the Moon disclosed a long stratum uniting them together at their bases, giving to the whole phenomenon the aspect of a *sierra* resting on the Moon's limb, and extending over an arc of about 20°. This continuous succession of prominences was intensely red, resembling in colour the most beautiful vermilion. The light was very much condensed, there being an entire absence of the streaks which characterized the larger prominences. The re-appearance of the Sun was preceded by an excessively narrow band of red light bounding the Moon's limb and connecting itself with the *sierra* previously observed. This long streak of light extended over an arc of about 25° on the Moon's limb. The reappearance of the first ray of the solar light resembled a bright star emerging at the Moon's limb. It occurred at 3^h 3^m 10^s." *

On an Universal Equatoreal. By John Browning, Esq.

Wishing to employ the telescope used by Colonel Tennant for photographing the Solar Eclipse of 1868, for the purpose of photographing the next Solar Eclipse in India, it occurred to the Astronomer Royal that it would be a great advantage to have the instrument so made that it might be considered universal, and used in any latitude between the equator and 60° to 70° N. or S. of the equator. When the instrument had been altered so as to fulfil these conditions, I believe to the satisfaction of the Astronomer Royal, I saw that a more efficient instrument might have been made if it had been planned specially for this purpose. I have prepared for exhibition before the Society a model of such an instrument, and I am now making one of 10½-in. diameter, for a gentleman at Sydney.

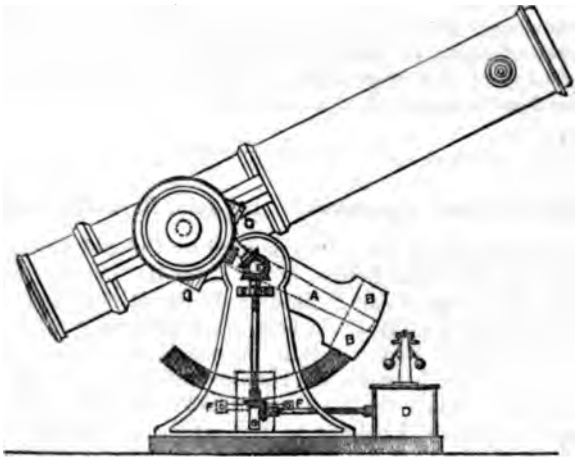
On reference to the diagram, the principle of construction will, I think, be evident.

The whole of the telescope, with the Polar axis, is carried by centres at E; large plates are attached to the sides of a cradle, which carries the Polar axis; AA is the Polar axis; BB the cradle in which it is carried; CC, an arc with a clamping arrangement, of which the adjusting screws are shown at FF; a clamp screw is hidden by the spur-wheels; when this screw is released, the angle of the Polar axis of the instrument may be changed from the horizontal to a nearly vertical position, or let to remain in any intermediate position between these, reclamped and finally adjusted by the capstan-screws FF; D is the clock, which, by means of a bevelled spur-wheel, gives motion to a wheel which revolves round the centre on which the Polar axis turns at E; this wheel, through the intermediation of two other wheels turns the driving-

* Greenwich Mean Time. The observations were made with a Dollond Refractor of 2¾ inches aperture.

screw on the hour-circle GG, and thus drives the instrument; as the wheel EE runs loose on the spindle, and the distance between the wheel driven by E, and the main driving-screw on the hour-circle remains unaltered, it is evident that a change in the angle of position of the Polar axis does not interfere so as to prevent the instrument being freely driven by the clock.

As I have before stated, the suggestion that such an instrument should be made, was the Astronomer Royal's; and I have received efficient assistance from Mr. De La Rue in perfecting this contrivance.



On the Motion of Matter projected from the Sun: with special reference to the Outburst witnessed by Prof. Young of America. By Richard A. Proctor, B.A. (Cambridge.)

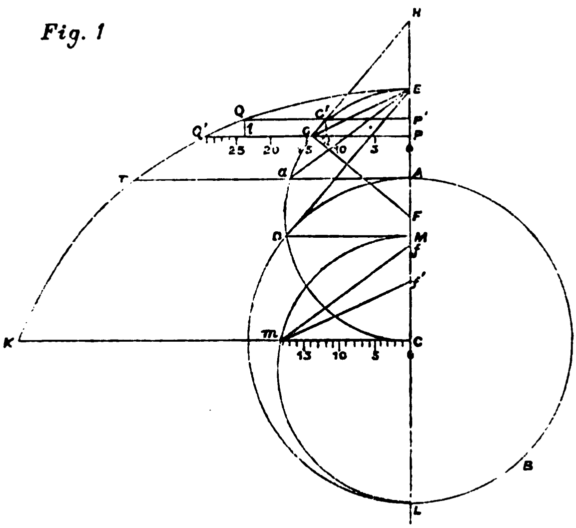
Whatever opinion we may form as to the way in which the matter of certain solar prominences is propelled from beneath the photosphere, there can be little question that such propulsion really takes place. It seems clear indeed that some prominences, more especially those seen in the Sun's polar and equatorial regions, are formed—or rather make their appearance—in the upper regions of the solar atmosphere, and even assume the appearance of eruption prominences by an extension *downwards*, somewhat as a waterspout simulates the appearance of an uprushing column of water though really formed by a descending movement. But it is certain that other prominences are really phenomena of eruption, because Respighi, Zöllner, and Young (probably also Lockyer and Secchi) have been able actually to watch the uprush of the flowing hydrogen.

In the case of any matter thus erupted, we shall clearly obtain

an inferior limit for the value of the initial velocity of outrush, if we assume that the apparent height reached by the matter is the real limit of its upward motion (that is, that there is no foreshortening) and that the solar atmosphere exercises no appreciable influence in retarding the motion. The latter supposition is, however, wholly untenable under the circumstances, while the former must in nearly all cases be erroneous; and I only make these suppositions in order to simplify the subject, noting that their effect is to reduce the estimated velocity of outrush to its lowest limiting value.

We are to deal then, for the present, with the case of matter flung vertically upwards from the Sun's surface and subject only to the influence of solar gravity; I propose to consider the time

Fig. 1



Illustrating the motion of a body descending from rest towards a globe attracting according to the law of nature.

of flight between certain observed levels, not the mere vertical distance attained by the erupted matter; and (as I wish to deal with cases where a great distance from the Sun has been attained) it will be necessary to take into account the different actions of solar gravity at different distances. Zöllner, in dealing with prominences of moderate height, has regarded the solar gravity as constant; but this is evidently not admissible when we come to deal with matter hurled to a height of 200,000 miles, since at that height solar gravity is reduced to less than one-half the value it has at the surface of the Sun.

It is easy to obtain the required formula; and though it is doubtless contained in all treatises on Dynamics, it will be as well to run through the work in this place. In reducing the formula

I have noticed a neat geometrical illustration (and partial proof) which I do not remember to have seen in any book. It not only presents in a striking manner the varying rate at which a body falls towards a centre attracting according to the law of Nature, but it supplies a means whereby the time of flight between any given distances may be readily obtained from a simple construction.

Let C be the centre of a globe A B D, of radius R, and attracting according to the law of nature; let gravity at the surface of the globe be represented by g. Then the attraction exerted at a unit of distance, if the whole mass of the globe were collected at a point, would be $g\,R^2$.

Let a particle falling from rest at E reach the point P in time t; and let $AE = H$, and $CP = x$. Then the equation of motion is

$$\frac{d^2 x}{d t^2} = -\frac{g\,R^2}{x^2},$$

giving

$$\left(\frac{d x}{d t}\right)^2 = v^2 = \frac{2\,g\,R^2}{x} + C;$$

so that, since the particle starts from rest at a distance $R + H$ from C, we have

$$0 = \frac{2\,g\,R^2}{R + H} + C.$$

For convenience write D for $R + H$; then we have

$$\left(\frac{d x}{d t}\right)^2 = v^2 = 2\,g\,R^2\left(\frac{1}{x} - \frac{1}{D}\right)$$

$$= \frac{2\,g\,R^2}{D}\left(\frac{D - x}{x}\right) \qquad (1)$$

Thus

$$R\sqrt{\frac{2\,g}{D}}\cdot\frac{d}{d x} = \frac{x}{\sqrt{D x - x^2}}.$$

Integrating we have

$$R\sqrt{\frac{2\,g}{D}}\cdot t = \sqrt{D x - x^2} - \frac{D}{2}\cos^{-1}\left(\frac{D - 2x}{D}\right) + C$$

But $t = 0$, $x = D$; so that $C = \frac{D\pi}{2}$,

hence we have

$$R\sqrt{\frac{2\,g}{D}}\,t = \sqrt{D x - x^2} + \frac{D}{2}\cos^{-1}\left(\frac{2x - D}{D}\right) \qquad (2)$$

(where D is equal to the radius of the globe added to the height from which the particle is let fall.)

Equation (1) gives the velocity acquired in falling (from rest) from a height H to a distance x from the centre, and (2) gives the time of falling to that distance. Now the geometrical illustration to which I have referred, relates to the deduction of (2) from (1). We see from (1) that at the point P

$$v^2 = \frac{2gR^2}{D}\left(\frac{D-x}{x}\right).$$

Bisect CE in F, and describe the semicircle CDE; then if DE is a tangent to the circle DAB, and if DM is drawn perpendicular to CE,

$$CM = \frac{CD^2}{CE} = \frac{R^2}{D};$$

so that

$$v = \sqrt{2g \cdot CM}\sqrt{\frac{PE}{CP}}. \qquad (\alpha)$$

But if close by G, either on the tangent GH or on the arc GE, we take G′ and draw G′P′ perpendicular to CE, and G′n perpendicular to GP, we have

$$\frac{GG' + Gn}{PP'} = \frac{GF + FP}{GP} = \frac{CP}{\sqrt{CP \cdot PE}}$$

$$= \sqrt{\frac{CP}{PE}}.$$

Hence, from (α),

$$\frac{v}{\sqrt{2g \cdot CM}} = \frac{PP'}{GG' + Gn},$$

so that

$$\left\{\begin{matrix}\text{the vel.}\\ \text{at P}\end{matrix}\right\} : \left\{\begin{matrix}\text{velocity acquired in falling through}\\ \text{space C M, under const. accel. force } g\end{matrix}\right\}$$

$$:: \left\{\begin{matrix}\text{elem. space}\\ \text{P P}'\end{matrix}\right\} : \left\{\begin{matrix}\text{sum of elementary}\\ \text{spaces G G}' \text{ and G} n.\end{matrix}\right\}$$

Therefore the falling particle traverses the space PP′, in the same time that a particle travelling with the velocity acquired in falling through space CM under constant accelerating force g, would traverse the space GG′+ Gn. It follows obviously that the time in falling from E to P is the same as would be occupied by a particle in traversing the space (arc EG + GP), with the

velocity acquired in falling through the space C M under a constant accelerating force g. This amounts to saying that

$$t = \frac{\text{P G} + \text{arc G E}}{\sqrt{2g \,.\, \text{C M}}},$$

or that

$$\text{R}\sqrt{\frac{2g}{\text{D}}} \,.\, t = \sqrt{\text{P E} \,.\, \text{P C}} + \text{C F arc G E}$$

$$= \sqrt{(\text{D}-x)\,x} + \frac{\text{D}}{2}\cos^{-1}\left(\frac{2x-\text{D}}{\text{D}}\right)$$

as before.

The relation here considered affords a very convenient construction for determining times of descent in any given cases. For, if P G be produced to Q so that G Q = arc G E, Q lies on a semi-cycloid K Q C, having C E as diameter; and the relative time of flight from E to any point in A E is at once indicated by drawing through the point an ordinate parallel to C K. The actual time of flight in any given case can also be readily indicated. For let T be the time in which L C would be described with the velocity acquired in falling through a distance equal to L C under accelerating force g; and on L M describe the semicircle L m M; then clearly C m ($= \sqrt{\text{C L} \,.\, \text{C M}}$) will be the space described in time T with the velocity acquired in falling through the space C M under accelerating force g; and we have only to divide C m into parts corresponding to the known time-interval T, and to measure off distances equal to these parts on P Q to find the time of traversing P Q with this uniform velocity, *i.e.*, the time in which the particle falls from E to P. The division in the figure illustrates such measurements in the case of the Sun, the value of T being 18⅝ minutes.

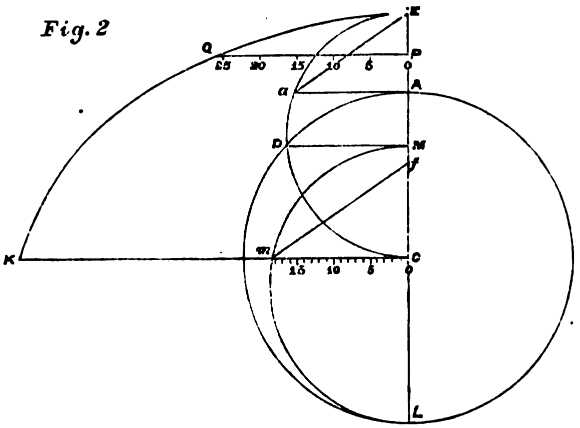
Moreover it is not necessary to construct a cycloid for each case. One carefully constructed cycloid * will serve for all cases, the radius C A being made the geometrical variable. As an instance of the method of construction, I will take Prof. Young's recent remarkable observation of a solar outburst, premising that I only give the constructions as illustrations, and that a proper calculation will follow.

Prof. Young saw wisps of hydrogen carried from a height of 100,000 miles to a height exceeding 200,000 miles from the Sun's surface in ten minutes. It is assumed here that there was no

* It is easy to construct a cycloid with great accuracy, nor need the methods available be here considered. But it is worthy of notice that if a wire coil be formed into a helix having the thread inclined 45° to the axis (which can be readily effected by the method which De Morgan suggested to Admiral Smyth,—see *Bedford Cycle*), and this thread placed on a horizontal table when the Sun is 45° high (the vertical plane including the Sun and the axis of the helix) the coil will throw a cycloidal shadow. (This arrangement is only presented in this way to show exactly how the point of projection should be placed with respect to the helix and plane; of course the projection can be obtained without the Sun's aid.)

foreshortening. The height, 100,000 miles, was determined by estimation; but the ultimate height reached by the hydrogen wisps (that is, the elevation at which they vanished as by a gradual dissolution) results from the mean of three carefully executed and closely accordant measures. This mean was 7′ 49″, corresponding to a height of 210,000 miles. We may safely take 100,000 miles as the vertical range actually traversed, and 200,000 miles as the extreme limit attained. We need not inquire whether the hydrogen wisps were themselves projected from the photosphere,—most probably they were not,—but if not, yet beyond question there was propelled from the Sun some matter which by its own motion caused the hydrogen to traverse the above-mentioned range in the time named. We shall be underrating the velocity of expulsion, in regarding this matter as something solid propelled through a non-resisting medium, and attaining an extreme range of 200,000 miles. What follows will show whether this supposition is admissible.

Fig. 2



Illustrating the construction for determining time of descent of a particle from rest towards a globe attracting according to the law of nature.

Now g for the Sun, with a mile as the unit of length and a second for the unit of time is ·169, and R for the Sun is 425,000. Thus the velocity acquired in traversing R under uniform force g

$$= \sqrt{2g \, . \, R}$$

$$= \sqrt{338 \times 425}$$

$$= 379, \text{ very nearly}$$

(this is also the velocity acquired under the Sun's actual attraction by a body moving from an infinite distance to the Sun's surface.)

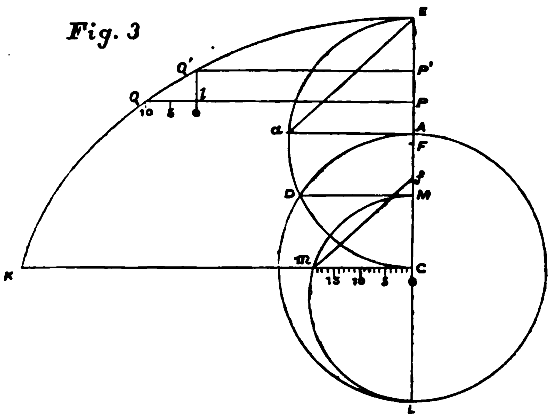
And a distance 425,000 would be traversed with this velocity in $18^m\ 40^s$ (= T.)

Let K Q E, *fig.* 2, be our semi-cycloid (available for many successive constructions if these be only pencilled), and C D E half the generating circle.

Then the following is our construction:—Divide E C into $6\frac{1}{4}$ equal portions, and let E P, P A be two of these parts, so that E A represents 200,000 miles and C A 425,000 miles (the Sun's radius). Describe the semicircle A D L about the centre C and draw D M perpendicular to E C; describe half circle M *m* L. Then *m* C represents T where the ordinate P Q represents time of falling from E to P.

$T = 18^h\ 50^m$, and P Q (carefully measured) is found to correspond to about twenty-six minutes.

Fig. 3



Illustrating the construction for determining time of descent between given levels, when a body descends from rest at a given height towards a globe attracting according to the law of nature.

Thus a body propelled upwards from A to E would traverse the distance P E in twenty-six minutes. But the hydrogen wisps watched by Prof. Young traversed the distance represented by P E in ten minutes. Hence either E was not the true limit of their upward motion, or they were retarded by the resistance of the solar atmosphere. Of course if their actual flight was to any extent foreshortened, we should only the more obviously be forced to adopt one or other of those conclusions.

But now let us suppose that the former is the correct solution; and let us inquire what change in the estimated limit of the uprush will give ten minutes as the time of moving (without resistance) from a height of 100,000 to a height of 200,000 miles. Here we shall feel the advantage of the constructive method; for to test the matter by calculation would be a long process, whereas each construction can be completed in a few minutes.

Let us try 375,000 miles as the vertical range. This gives C E = 800,000 miles, and our construction assumes the following appearance. We have A C to represent 425,000; A P = P P′ = 100,000 miles; and Q l or (P Q − P′ Q′) to represent the time of flight from P to P′.

The semicircles A D L, M m L give us m C to represent T or $18^h\ 50^m$; and Q L carefully measured is found to correspond to rather less than ten minutes. It is, however, near enough for our purpose.

It appears, then, that if we set aside the probability, or rather the certainty, that the Sun's atmosphere exerts a retarding influence, we must infer that the matter projected from the Sun reached a height of 375,000 miles, or thereabouts. This implies an initial velocity of about 265 miles per second.*

But it will be well to make an exact calculation,—not that any very great nicety of calculation is really required, but —to illustrate the method to be employed in such cases, as well as to confirm the accuracy of the above constructions.

In equation (2) put $\sqrt{2\,g\,R} = 379$; R = 425,000; D = 625,000; and x = 525,000; values corresponding to Prof. Young's observations. It thus becomes—

$$\sqrt{\frac{425}{625}}\,(379)\,t = \sqrt{(100{,}000)\,(525{,}000)} + 312{,}500 \cos^{-1}\left(\frac{1050-625}{625}\right),$$

or

$$379\sqrt{17}\,.\,t = 250{,}000\sqrt{21} + 1{,}562{,}500 \cos^{-1}\left(\frac{17}{25}\right),$$

$$1562{\cdot}7\,t = 1{,}145{,}100 + 1{,}285{,}800 = 2{,}430{,}900$$

$$t = 1556^s = 25^m\ 56^s.$$

This then is the time which would have been occupied in the flight of matter from a height of 100,000 to a height of 200,000 miles, if the latter height had been the limit of vertical propulsion in a non-resisting medium.

In order to deduce the time of flight t' between the same levels, for the case where the total vertical range is 375,000 miles, we have, putting t_1 for time of *fall* to 200,000 miles above Sun's surface, and t_2 for time of fall to 100,000 miles, the equation,

* This value is of course deduced directly from (1); but it is worthy of notice that it can be deduced at once from *fig.* 3, by drawing A a parallel to K C, and $m\,f$ parallel to a E; then C f represents the required velocity, C L representing 379 miles per second. A similar construction will give the velocity at P, P′ &c. Applied to *fig.* 1, it gives C f to represent the velocity at A, C f' to represent the velocity at P; $m\,f$ and $m\,f'$ being parallel to a E and G E respectively. Applied to the case dealt with in *fig.* 2, we get C f to represent the velocity at A, where E is the limit of flight; C f is found to be rather more than $\frac{5}{9}$ of C L; so that the velocity at A is rather more than 210 miles per second.

$$\sqrt{\frac{425}{800}}\,(379)\,t_1 = \sqrt{(175{,}000)\,(625{,}000)} + (400{,}000)\cos^{-1}\left(\frac{125-80}{80}\right),$$

and

$$\sqrt{\frac{425}{800}}\,(379)\,t_2 = \sqrt{(275{,}000)\,(525{,}000)} + (400{,}000)\cos^{-1}\left(\frac{105-80}{80}\right),$$

giving

(since $t_2 - t_1 = t'$)

$$\sqrt{\frac{425}{800}}\,(379)\,t' = 25{,}000\left\{\sqrt{11\times 21} - \sqrt{7\times 25}\right\}$$
$$+\,400{,}000\left\{\cos^{-1}\left(\frac{5}{16}\right) - \cos^{-1}\left(\frac{9}{16}\right)\right\}$$

$$276{\cdot}25\,t' = 49{,}250 + 111{,}816 = 161{,}066,$$

$$t' = 583^{s} = 9^{m}\,43^{s}.$$

This is very near to Prof. Young's ten minutes. I had found that an extreme height of 400,000 miles gave $9^{m}\,24^{s}$ for the time of flight between vertical altitudes 100,000 miles and 200,000 miles. It will be found that a height of 360,000 miles gives $9^{m}{\cdot}585$, which is sufficiently near to Prof. Young's time.

Now to attain a height of 360,000 miles a projectile from the Sun's surface must have an initial velocity

$$= \sqrt{2gR}\,.\,\sqrt{\frac{360{,}000}{785{,}000}} = 379\sqrt{\frac{72}{157}}$$

$$= 257 \text{ miles per second.}$$

The eruptive velocity, then, at the Sun's surface, cannot possibly have been less than this. When we consider, however, that the observed uprushing matter was vapourous, and not very greatly compressed (for otherwise the spectrum of the hydrogen would have been continuous and the spectroscope would have given no indications of the phenomenon), we cannot but believe that the resisting action of the solar atmosphere must have enormously reduced the velocity of uprush before a height of 100,000 miles was attained, as well as during the observed motion to the height of 200,000 miles. It would be safer indeed to assume that the initial velocity was a considerable multiple of the above-mentioned velocity, than only in excess of it by some moderate proportion. Those who are acquainted with the action of our own

atmosphere on the flight of cannon-balls (whereby the range becomes a mere fraction of that due to the velocity of propulsion), will be ready to admit that hydrogen rushing through 100,000 miles even of a rare atmosphere, with a velocity so great as to leave a residue sufficient to carry the hydrogen 100,000 miles in the next ten minutes, must have been propelled from the Sun's surface with a velocity many times exceeding 257 miles per second, the result calculated for an unresisted projectile. Nor need we wonder that the spectroscope supplies no evidence of such velocities, since when motions so rapid existed, others of all degrees of rapidity down to such comparatively moderate velocities as twenty or thirty miles per second would also exist, and the spectral lines of the hydrogen so moving would be too greatly widened to be discerned.

Now the point to be specially noticed is, that supposing matter more condensed than the upflung hydrogen to be propelled from the Sun during these eruptions, such matter would retain a much larger proportion of the velocity originally imparted. Setting the velocity of outrush, in the case we have been considering, at only twice the amount deduced on the hypothesis of no resistance (and it is incredible that the proportion can be so small), we have a velocity of projection of more than 500 miles per second; and if the more condensed erupted matter retained but that portion of its velocity corresponding to three-fourths of this initial velocity (which may fairly be admitted when we are supposing the hydrogen to retain the portion corresponding to so much as half of the initial velocity), then such more condensed erupted matter would pass away from the Sun's rule never to return.

The question may suggest itself, however, whether the eruption witnessed by Prof. Young might not have been a wholly exceptional phenomenon, and so the inference respecting the possible extrusion of matter from the Sun's globe be admissible only as relating to occasions few and far between. Now on this point I would remark, in the first place, that an eruption very much less noteworthy would fairly authorise the inference that matter had been ejected from the Sun. I can scarcely conceive that the eruptions witnessed quite frequently by Respighi, Secchi, and Young—such eruptions as suffice to carry hydrogen 80,000 or 100,000 miles from the Sun's surface—can be accounted for without admitting a velocity of outrush exceeding considerably the 379 miles per second necessary for the actual *rejection* of matter from the Sun. But apart from this it should be remembered that we see only those prominences which happen to lie round the rim of the Sun's visible disk, and that thus many mighty eruptions must escape our notice even though we could keep a continual watch upon the whole circle of the sierra and prominences (which unfortunately is very far from being the case).

It is worthy of notice that the great outrush witnessed by Prof. Young was not accompanied by any marked signs of mag-

netic disturbance. Five hours later, however, a magnetic storm began suddenly, which lasted for more than a day; and on the evening of September 7 there was a display of the aurora borealis. Whether the occurrence of these signs of magnetic disturbance were associated with the appearance (on the visible half of the Sun) of the great spot which was approaching or crossing the eastern limb at the time of Young's observation, cannot at present be determined.

I would remark, however, that so far as is yet known the disturbance of terrestrial magnetism by solar influences would appear to depend on the condition of the photosphere, and therefore to be only associated with the occurrence of great eruptions in so far as these affect the condition of the photosphere. In this case an eruption occurring close by the limb could not be expected to exercise any great influence on the Earth's magnetism; and if the scene of the eruption were beyond the limb, however slightly, we could not expect any magnetic disturbance at all, though the phenomena of eruption might be extremely magnificent.

In this connection I venture to quote from a letter addressed to me by Sir J. Herschel in March 1871 (a few weeks only before his lamented decease). The letter bears throughout on the subject of this paper, and therefore I quote more than relates to the association between terrestrial magnetism and disturbances of the solar photosphere.

After referring to Mr. Brothers' photograph of the corona (and remarking by the way that "the corona is certainly *extra*-atmospheric and *ultra*-lunar"), Sir John Herschel proceeds thus:—

"I can very well conceive great outbursts of vaporous matter from below the photosphere, and can admit at least the possibility of such vapour being tossed up to very great heights; but I am hardly yet exalted to such a point as to conceive a positive ejection of erupted particles with a velocity of two or three hundred miles per second. But now the great question of all arises: what *is* the *photosphere?* what are those intensely radiant *things*—scales, flakes, or whatever else they be—which really *do* give out all (or at least $\frac{99}{100}$ths of) the total light and heat of the Sun? and if the prominences &c. be eruptive, why does not the eruptive force scatter upwards and outwards this luminous matter? .... Through the kindness of the Kew observers I have had heliographs of the two great outbursting spots which I think I mentioned to you as having been non-existent on the 9th, and in full development on the 10th—both [being] large and conspicuous, and including an area of disturbance at least 2′ (54,000 miles) across. They were both nearly absorbed, or in rapid process of absorption, on the 11th. In my own mind I had set it down as pretty certain that the outbreak must have taken place *very* suddenly at somewhere about the intervening midnight. Well, now! The magnet's declination curves at Kew have been sent me, and, lo! while they had been going on as

... 7th, 8th, and 9th, and up to ... suddenly a great downward jerk ... as $3\frac{1}{2}$ A.M. on the 10th. Then ... A.M., and then (corresponding to ... furious and convulsive state of ... 11th and the greater part of the ... was shot out of those holes on ... is going on in the inside of the

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... the determination of the Elements of ... Meridional Observations of the Moon. ... Airy, Astronomer Royal.

... the Moon's apparent disk produces a diffi- ... of observations of the Moon which does not ... the Sun or any of the Planets. In general, ... limbs of the Moon (either those observed in ... those observed in North Polar Distance) can- ... minated, and, in the great majority of instances, ... be in a state approximating to that of full illu- ... attempt can be made to observe the centre; and ... only way of determining the apparent place of the ... observe the fully illuminated limb, and to apply to ... numerical correction representing the computed value ... semidiameter at the time of observation. This appa- ... meter must be computed from a mean semidiameter ... om observations of both limbs, when both limbs are so ... the state of full illumination, that the small correction ... on the deficiency of illumination of one limb can be ... with great accuracy. Very few observatories furnish ... ions adapted to this purpose; and in consequence there ... ways be a little uncertainty about the value of the apparent ... ameter.

... correcting the observed Right Ascension of a limb, there ... therefore always be a small uncertainty on the inferred Right ... ension of the centre, which has one sign before Full Moon ... the opposite sign after Full Moon. It is well known that ... is introduces uncertainty with regard to the coefficients of the ... two important equations designated the "Variation" and the "Parallactic Equation." These effects, however, are sufficiently well known, and I shall not delay further on them.

The point to which I invite the attention of the Society is one which I believe has never been noticed, namely, the effect of the uncertainty of correction for semidiameter on the inf... for North Polar Distance of the Moon's centre. I b... is not generally known to the investigators of Lun...

the constructors of Lunar Tables (it was not known to me till I had been a practical astronomer of some years' standing), that near the summer solstice, or (roughly) from the beginning of May to the end of July, only the North Limb of the Moon can be observed; and near the winter solstice, or from the beginning of November to the end of January, only the South Limb can be observed. At other seasons of the year, the north-limb-illuminations and the south-limb-illuminations are mingled in a singular way. I propose now to trace the general effect of the uncertainty in question, as limited in its application by the peculiarity of observation which I have described.

Suppose that a semidiametral correction larger than that which is just is applied to all observations of North Polar Distance. When the north limb is observed, this will increase the inferred North Polar Distance of the centre, and this applies to the summer observations. When the south limb is observed, the supposed too-large correction will diminish the inferred North Polar Distance of the centre; and this applies to the winter observations. Therefore the plane of the Moon's orbit is made to approach too near to the plane of the terrestrial equator. When the Moon's ascending node is at the first point of *Aries*, the inclination of her orbit to the ecliptic is thus diminished; and when the ascending node is at the first point of *Libra*, this inclination to the ecliptic is increased. Thus there is made to appear, in the value of the Inclination of the Moon's Orbit, a periodical change whose period is 19·2 years (the same as the period of nutation). The same result, but with opposite sign, occurs if the employed semidiametral correction is too small.

I have made great efforts to reduce the extent of this uncertainty. Since I have had the command of an Observatory, I have always carefully prepared the plan of observations of each following year before the end of the preceding year. In doing this at the Royal Observatory, a calculation is made for every day on which the Moon can be observed on the meridian, showing the state of the Moon's illumination; and from this, the possibility of using double observations of the limbs for the correction of semidiameters is immediately seen. The number available in Right Ascension is very small; the number of those which can be used in North Polar Distance is larger. I have laid down the rule, that the observations to be employed shall only be those of the gibbous Moon, and that the defect of illumination upon the imperfect limb of the Moon shall not exceed three degrees of the Moon's surface. The results of these are given in the annual volumes of *Greenwich Observations*, and it will be seen there that a considerable amount of information on this important element has been collected.

In my discussion of the Corrections to the Elements of the Moon's Orbit, as inferred from the observations made at Greenwich from 1750 to 1830, printed in Volume XVII. of the Society's *Memoirs*, I have found on page 54, as one of the results obtained

by examination of the observations of N.P.D., an inequality in the inclination expressed by

$$-\ 2''\cdot 02 \times \text{sine longitude of node.}$$

And in the extension of this discussion, including the results of observations from 1750 to 1851, printed in Volume XXIX. of the *Memoirs*, I have found on page 21,

$$-\ 1''\cdot 87 \times \text{sine longitude of node.}$$

It is very probable that these terms may have originated, at least in part, in erroneous value of the semidiameter employed in the reductions.

It is proper to remark that any important error in refraction, applying with different magnitudes to the summer observations and the winter observations, will mix its effects with those of erroneous semidiameter.

Royal Observatory, Greenwich,
1871, *November* 15.

On the Geodesic Lines on an Ellipsoid. By Prof. Cayley.
(Abstract.)

The fundamental equations in regard to the geodesic lines on an ellipsoid were established by Jacobi, viz., representing by a, b, c, the squares of the semiaxes, that is, taking the ellipsoid to be

$$\frac{x^2}{a} + \frac{y^2}{b} + \frac{z^2}{c} = 1,$$

(where $a > b > c$), if we introduce the elliptic co-ordinates h, k, and write

$$\frac{x^2}{a+h} + \frac{y^2}{b+h} + \frac{z^2}{c+h} = 1,$$

$$\frac{x^2}{a+k} + \frac{y^2}{b+k} + \frac{z^2}{c+k} = 1.$$

or, what is the same thing,

$$x^2 = \frac{a\,(a+h)(a+k)}{(a-b)(a-c)},$$

$$y^2 = \frac{b\,(b+h)(b+k)}{(b-c)(b-a)},$$

$$z^2 = \frac{c\,(c+h)(c+k)}{(c-a)(c-b)},$$

then, if β be an arbitrary constant, the differential equation of a geodesic line is

$$(1). \text{ Const.} = \int dh \sqrt{\frac{h}{(a+h)(b+h)(c+h)(\beta+h)}} + \int dk \sqrt{\frac{k}{(a+k)(b+k)(c+k)(\beta+k)}},$$

and the expression for the length of any arc of the curve is given by

$$(2). \; s = \int dh \sqrt{\frac{h(\beta+h)}{(a+h)(b+h)(c+h)}} + \int dk \sqrt{\frac{k(\beta+k)}{(a+k)(b+k)(c+k)}}.$$

I propose in the present memoir to develope the theory to the extent of showing how we can, by means of the first of these equations, explain the course of the geodesic lines; and for given numerical values of a, b, c, calculate, construct, and exhibit in a drawing, the course of these lines. I attend more particularly to the series of geodesic lines through an umbilicus (which lines pass also through the opposite umbilicus) and to the case where the semiaxes are connected by the equation $ac - b^2 = 0$, a relation which simplifies the formulæ.

On the Floor of Plato. By W. R. Birt, Esq.

I have just completed an examination of 133 observations of the tints of the floor of *Plato* by different observers, which has yielded the accompanying curve, and thinking it might be interesting to the Society, I have the pleasure of forwarding it for communication. The observations have been made in accordance with the annexed Form, a light tint having a value of 0·33, medium 0·50, and dark 0·66, higher or lower values have been used to express a very dark or a very light floor. It has long been known that the grey plains appear darkest under a high Sun, but I am not aware that any attempt has hitherto been made to exhibit graphically the progression of tint, and to show its connection with that of the Sun's altitude, which the two curves do unmistakably. The dotted curve is that of the Sun's altitude on the parallel of 50° at the equinoxes, the continuous curve that of the progression of tint, the inflexions of which indicate rather considerable variability, especially in the deepening of the tint. One remarkable feature is, that the chromatic curve lies almost wholly within that of altitude, from which it may be inferred that the full effect of the Sun's influence is scarcely attained, and that the average state of the floor of *Plato* is somewhat *lighter* than it

would be under solar influence alone. It is desirable that observations of this kind should be multiplied, but, from the nature

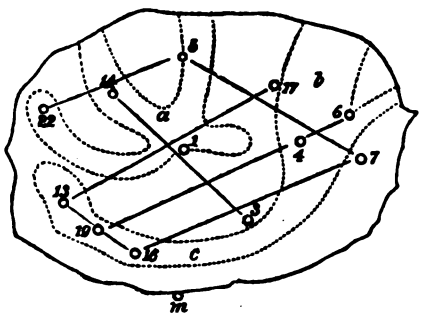
of the investigation, some years must elapse before our present vague and indefinite knowledge can become certain.

Cynthia Villa, Walthamstow, November 25, 1871.

Lunar Map VII.

Plato.

Spots and markings on floor, 1869, August 26.—H. Pratt.



Instructions.—*First*, note the appearance of the border and objects immediately contiguous, if sharp and well-defined, or indistinct and hazy; also the tint of floor, light, medium, or dark. *Second*, record particularly the relative brightness and definition of Nos. 1, 4, 3, and 17, and whether they appear as craterlets or spots. *Third*, give the relative brightness of the remaining spots observed. If a spot be unobserved, write against it " not seen."

Observations. Date

Instrument Power

Border

Floor

No. 1.
" 3.
" 4.
" 5.
" 6.
" 7.
*" 13.
" 14.
" 14.
" 16.
" 17.
*" 19.
*" 22.

* These spots should be particularly sought for.

Remarks

Observer

Lunar Occultations and Eclipses of Jupiter's Satellites. By John Tebbutt, Esq.

I herewith send a list of lunar occultations and eclipses of *Jupiter's* satellites observed by me at Windsor. The first-mentioned phenomena were not previously computed, so that I was unable, except in two or three instances, to observe the reappearances. A few only of the stars are to be found in the Occultation-List of the *Nautical Almanac*, or in the Catalogues in my possession; but the magnitude of each star and its roughly estimated position, when in contact with the Moon's limb, are given in order to facilitate its future identification. With the exception of the observation of May 18, 1864, all the occultations were observed with my 4-feet refractor and a power of 30. The eclipses of *Jupiter's* satellites were observed with the same telescope, but with a power of 120. The chronometer on the 12th April and 18th May, 1864, was regulated from sextant observations, but on all other occasions with the transit-instrument. It is hoped that the list of occultations may hereafter prove useful for the accurate determination of the longitude of the Observatory, and hence serve for a verification of that of the Sydney Observatory. In conclusion, I may state that the latitude of my Observatory, as finally deduced from a large number of prime vertical observations of stars whose positions have been accurately determined in southern observatories, is $-33^\circ\ 36'\ 29''\cdot 2$.

Paramatta, N.S. Wales, Sept. 26th, 1871.

Occultations, 1864–70.

Date of Obs.		Mag. of Stars.	Disappearance or Reappearance.	Windsor Mean Time.	Angle from Moon's N. Cusp.	Remarks.
1864.				h m s	°	
April	12	4½	D	8 46 11·6	80 E	Excellent observation Disappearance sudden.
May	18	1	D	13 39 16·5	—	Disappearance instantaneous. Obs. made with telescope of 1¾-in. aperture.
1866.						
April	18	6½	D	6 14 36·2	70 E	Obs. uncertain to 1ˢ, star being faint.
May	17	6½	D	7 2 12·2	80 E	Star appeared to encroach slightly on disk before extinction.
	19	7½	D	6 22 52·8	120 E	Star very faint at disappearance.
	19	6½	D	6 52 49·8	40 E	Star distinct, but telescope tremulous.
	19	8	D	8 48 39·8	70 E	Star distinct.
	22	6½	D	8 13 50·2	75 E	Good observation.
Sept.	24	7	D	11 31 7·7	—	Extinction remarkably sudden, being observed near N. limb during total eclipse.
Oct.	15	6½	D	7 21 22·3	100 E	Disappearance instantaneous.
1867.						
May	9	6½	D	6 31 38·4	90 E	Disappearance not quite instantaneous.
Oct.	1	6½	D	7 27 51·4	90 E	Star rather faint.
1868.						
Feb.	27	5½	D	7 48 46·9	80 E	Good observation.
March	2	5	D	10 11 27·2	30 E	Good observation. Disappearance gradual.
May	26	7	D	7 37 36·1	150 E	
	26	8	D	7 48 57·6	135 E	Star rather faint.
	26	8	D	7 53 31·6	60 E	Star rendered faint by cloud.
	27	5	D	7 35 1·6	135 E	Moon's limb exceedingly distinct and well defined.
	27	7	D	7 42 13·9	110 E	
	27	7½	D	7 45 32·9	90 E	
	27	7½	D	7 57 54·9	135 E	
	29	7	D	7 0 0·7	90 E	Observation uncertain owing to faintness of star on limb.
June	1	6	D	6 35 14·4	30 E	Disappearance rather gradual.
	1	6	D	7 24 44·4	130 E	Ditto more sudden.
Aug.	24	7	D	6 49 54·8	100 E	Good observation.
	24	7	D	7 20 56·1	85 E	Good observation.
	27	7	D	6 38 43·4	90 E	Star faint at disappearance.
	27	6½	D	6 46 14·0	70 E	
	27	6½	D	7 25 8·8	60 E	Disappearance uncertain to 1ˢ.
	27	6½	D	7 36 19·8	115 E	Disappearance gradual.
	27	6½	D	7 52 29·3	120 E	Disappearance sudden.
	27	7	D	7 56 20·0	120 E	Disappearance rather sudden.
	27	6	D	8 22 19·3	25 E	Disappearance uncertain owing to thin cloud.
	28	5½	D	8 1 37·7	70 E	Star probably disappeared about a quarter of a second earlier than time noted.
	28	6	D	9 26 52·6	90 E	Obs. 4 or 5 seconds late owing to temporary removal of eye from telescope.
	28	6	D	9 40 16·0	160 E	Disappearance gradual and star faint.
Sept.	22	6½	D	7 21 34·1	60 E	Good obs. disappearance sudden.
	26	6	D	8 3 41·2	90 E	Disappearance gradual.

Date of Obs.		Mag. of Stars.	Disappearance or Reappearance.	Windsor Mean Time.	Angle from Moon's N. Cusp.	Remarks.
1868.				h m s	°	
Oct.	22	7	D	6 54 50·7	135 E	Star clung to limb for some time.
	22	7	D	7 9 33·0	130 E	Disappearance rather gradual.
	22	6	D	8 12 7·5	150 E	Ditto.
	22	5½	D	8 13 22·0	65 E	Ditto.
	22	5½	D	8 27 18·9	75 E	Disappearance pretty sudden.
	27	6½	D	9 39 49·0	140 E	Star faint; disappearance rather gradual.
1869.						
Jan.	20	7	D	8 30 56·6	90 E	Star rather faint at disappearance.
Feb.	17	4	D	8 3 32·6	80 E	Disappearance sudden; very good obs.
	24	4	D	8 0 49·3	55 E	Good observation; disappearance sudden.
Mar.	22	5	D	10 2 5·0	70 E	Very good observation; disappearance very sudden.
Apr.	19	5	D	8 10 59·5	70 E	
	19	6	D	8 55 56·4	70 E	
Nov.	9	5½	D	9 6 52·0	80 E	Disappearance sudden, but followed by a faint flash of light.
Nov.	9	6½	D	9 46 45·8	135 E	Star faint, but disappearance sudden.
Dec.	6	7	D	7 43 56·1	25 E	Star faint; disappearance gradual.
	6	7	D	8 2 12·0	65 E	Star rather faint disappearance sudden.
	6	7	D	8 46 20·4	110 E	Star faint; disappearance uncertain to a second.
	10	6½	D	8 37 45·4	70 E	Disappearance not quite instantaneous.
1870.						
Jan.	17	7	D	11 59 24·7	—	Disappearance instantaneous.
	17	–	D	12 49½	—	Faint star.
	17	8	D	13 2 19·3	—	Star faint; time somewhat uncertain.
	17	8	D	13 5 0·2	—	Ditto.
	17	8	R	13 6 18·7	—	
	17	7	R	13 25 4·2	—	Recorded time rather late.
	17	7	D	13 36 24·2	—	Time uncertain to a second. All the occultations were observed during total eclipse.
Feb.	8	5	D	9 9 36·3	60 E	Disappearance sudden; observation very exact.
	11	3½	D	8 18 1·5	75 E	Disappearance instantaneous.
May	7	7½	D	6 1 58·3	70 E	Disappearance rather sudden.
June	3	5	D	7 19 52·0	70 E	Excellent observation; disappearance instantaneous.
	9	6½	D	6 43 38·4	25 E	Good obs.; disappearance rather gradual.
July	4	8	D	6 22 51·9	160 E	Star faint; disappearance gradual. Time uncertain to half a second.
Oct.	1	7½	D	7 40 14·1	110 E	Star faint at disappearance.
	1	7	D	7 42 23·6	100 E	Ditto.
	1	5	D	7 58 18·0	15 E	Disappearance instantaneous. Faint at reappearance and time probably 2 or 3 seconds late.
	1	5	R	8 11 35·5	—	
	28	6½	D	8 46 58·8	135 E	Disappearance instantaneous.
	29	6	D	7 19 45·3	145 E	Good observations, and disappearances instantaneous.
	29	6	D	8 52 45·4	140 E	
Nov.	28	7	D	8 36 51·7	40 E	Star rather faint at disappearance.
	30	4	D	10 7 52·2	80 E	Observation uncertain owing to a passing cloud.

Eclipses of Jupiter's Satellites, 1868–70.

Date of Obs.	Satellite.	Disappearance or Reappearance.	Windsor Mean Time.	Remarks.
1868.			h m s	
Aug. 25	II	D	11 17 28·8	Images steady and well defined. Moon present.
Oct. 22	I	R	8 44 16·5	Images tremulous and ill defined.
Nov. 7	I	R	7 4 33·1	Images rather tremulous; twilight had scarcely disappeared.
Dec. 10	III	D	9 39 2·4	Definition pretty good; sky rather hazy. Disappearance very gradual.
17	II	R	7 12 36·5	Strong twilight. Sun's upper limb disappeared on horizon 10 minutes previously. Images tremulous, but pretty well defined.
1869.				
Jan. 25	II	R	9 32 54·0	Planet only about 5° above horizon; images unsteady and ill defined.
Nov. 26	I	R	8 48 34·4	Images well defined; recorded time somewhat late.
1870.				
Jan. 1	III	D	11 8 42·1	Planet very ill defined and boiling.
Feb. 3	I	R	9 38 0·3	Limb boiling.
Dec. 6	I	D	11 56 41·2	Excellent observation.

Reply to the Notes and Queries made by the Astronomer Royal, on the "Observations of η Argûs and its Nebula," Monthly Notices R. A. S. for June 9, 1871, *pages* 233 *and* 234. By F. Abbott, Esq.

Note No. 1.—" See in particular the line of four stars convex towards η *Argûs.*"

These stars have retained their apparent positions, &c. more than any others in the same object, although variations to a small extent have taken place. In carefully looking over the drawings it will be found that a similarity exists in the position of many of the stars, but as a rule some changes have taken place.

In regard to the use of geometry, it was never my intention to apply it, but only to sketch the object as an eye-draft; and by adopting the term "line of sight," it was intended to simplify, by always observing the object in one position, *i.e.* the telescope lying in a direct line W.N.W. to E.S.E. (the drawing being reversed) and at an angle of from 70° to 80° above the horizon, according to the state of the atmosphere at the time. *In this line of sight* all the drawings have been made; and in an easy, sitting position, with the light shaded from everything but the paper, the object has always been carefully delineated.

With regard to "all the stars agreeing with either Sir John, or Lieut. A. Herschel's configurations," I never expected they

would, as apparent changes have more or less constantly been taking place.

The question asked in No. 7—"The drawing (Lieut. Herschel's) had undoubtedly reached Australia, has Mr. Abbott copied it?"—I answer, No!—The whole of my drawings were made previous to, and independent of, any others.

In making comparison, it would be desirable to refer to the original drawings, as in the lithographs, which are on a reduced scale, some trifling inaccuracies occur.

Private Observatory, Hobart Town,
5th October, 1871.

I have forwarded by the same mail lithographic copies of the drawings, made by order of the Government, and published in the *Proceedings of the Royal Society of Tasmania.*

Note on Mr. Abbott's imagined Discovery of great Changes in the Argo Nebula. By R. A. Proctor, B.A.

I have received a letter and several papers from Mr. Abbott on the subject of the Nebula in *Argo*; and he appears to desire that I should express an opinion as to the reality of the supposed changes in the Nebula. I have, therefore, re-examined his papers and pictures, to which I had given a careful scrutiny two or three years ago. I had then been quite perplexed by the arrangement of the stars as compared with the Cape Monograph. But now, after reading Capt. Herschel's Note on the subject in the last Supplementary Number of the *Monthly Notices*, I find not the least difficulty in interpreting Mr. Abbott's pictures. Unquestionably Capt. Herschel is right. Mr. Abbott has supposed the dark spaces (shown in Sir J. Herschel's Monograph) to correspond to the lemniscate, which would unquestionably imply a complete change in the whole aspect of the Nebula. On the scale of Mr. Abbott's drawings the lemniscate would be about 2-5ths of an inch long; it would, in fact, be a minute and scarcely discernible feature. Mr. Abbott's field of view has a diameter exceeding the length of the rectangular space included in Sir J. Herschel's Monograph. The case is precisely as though an observer with a small telescope, using a power so low as to give a field 1° 8′ in diameter, were to regard the appearance of the Nebula so seen as a view of the region immediately surrounding the trapezium. It is most unfortunate that by this mistake Mr. Abbott has caused so much valuable time to be wasted by Sir J. Herschel, the Astronomer Royal, Mr. Lassell, and others.

On the Spectrum of Hydrogen at Low Pressure.
By G. M. Seabroke, Esq.

During the late summer months I have been comparing the lines given by hydrogen in the spectroscope with the lines of the solar spectrum, for the purpose of ascertaining whether any lines in the Sun's chromosphere were due to hydrogen, besides those usually supposed to be due to this element. The observations are as yet incomplete; but as it will be some months before I can again proceed, I therefore produce the results obtained up to the present time. The experiments have been conducted in a room adjoining the Temple Observatory lately erected at Rugby. My mode of proceeding has been briefly as follows:—I use a vacuum tube containing hydrogen, and connected with a Sprengel's air-pump. The tube is of the ordinary form, having the part between the bulbs, into which the platinum wires pass, about $\frac{1}{40}$-inch internal diameter. The pressure in the tube varied from 3 to 4 mm of mercury. Preliminary experiments showed that at this pressure the lines appeared most distinct; but a slight change of pressure near 4 mm made little alteration in the lines. There is a battery of 12 Smees to work the coil for passing the spark in the vacuum tube. The light from the hydrogen tube passed through a lens which concentrates it on the slit of the spectroscope. A dispersive power of 4 prisms of 60° was used, the arrangement of the instrument being such that the ray of light traverses each prism twice. The room is kept perfectly dark, and sunlight is reflected down from the roof by means of a heliostat. At first I tried the usual mode of comparing spectra, viz. by having the hydrogen and solar spectra side by side. I found this answer very well for the bright lines, but the faint ones could not be distinguished by the side of the bright solar spectrum. I therefore placed a very fine platinum wire in the eyepiece of the spectroscope, and brought the lines under examination into coincidence with the wire, and then passed the sunlight in, and found which black line coincided with the wire, or, where there was no coincidence, the position of the wire with respect to the black lines. I have made from 10 to 20 observations on each line that I have at present examined. I have every reason to believe that the limit of error is within two divisions of Fraunhofer's scale either way. The table below gives the positions of the lines I have already compared. These I hope to examine again next year, and also to finish the remainder.

Reference No.	Position on Kirchhoff's Scale.	Relative Brightness. 10=brightest.	Remarks.
1	694	10	C.
2	881	..	Limit of a number of close lines towards C.
3	930	5	Brightest red line, except C.
4	1014	5	Suspiciously near the chromosphere line near D.

Reference No.	Position on Kirchhoff's Scale.	Relative Brightness. 10=brightest.	Remarks.
5	1049	3	The positions of these were taken by reference to the mercury lines, and are therefore not so reliable as the others.
6	1061	4	
7	1119	3	
8	1533	4	
9	1621	4	
10	1876	4	
11	1943	3	
12	1991	6	
13	2065	3	
14	2080	10	F.
15	2235	4	Near here, exact place uncertain.
16	2361	6	
17	2428·5	..	Limit of a band towards F.
18	2540	2	
19	2605	3	
20	2670	3	
21	2767	..	Faint band.

On comparing the above table with the catalogue of chromospheric lines by Professor Young, published in the *Philosophical Magazine* of November, 1871, I see no sufficient signs of coincidence to lead me to believe that any of the chromospheric lines in his list are due to hydrogen, except the C and F, already well known to be so due. Since I have not yet examined lines further than 2767, the "near G" (2796) and *h* lines, also known to be due to hydrogen, are not mentioned in the above list. In these experiments a spectroscope with a large dispersive and magnifying power was found to be required in order to identify the lines in the solar spectrum, so that the hydrogen spectrum became so reduced in brightness that, in order to see the fainter lines, the eye required to be kept for some minutes in the dark room, although with a spectroscope of low power the spectrum appeared very bright and full of lines.

Occultation of ι Capricorni by the Moon, Nov. 18th, 1871, observed at Forest Lodge, Maresfield. By Capt. William Noble.

The star disappeared instantaneously at the

Moon's dark limb at $22^h\ 52^m\ 31^s{\cdot}3$ L.S.T. = $7^h\ 3^m\ 0^s{\cdot}7$ L.M.T.

and reappeared, pretty sharply, at the

Bright limb about $23^h\ 39^m\ 8^s{\cdot}0$ L.S.T. = $7^h\ 49^m\ 58^s{\cdot}9$ L.M.T.

The Moon was low, and there was a good deal of undulation. Power 255, adjusted on the star.

An Eclipse of Jupiter's 3rd Satellite, December 4, 1871. By Capt. William Noble.

It is impossible to say at what instant the satellite first commenced to diminish in brightness; but it certainly began to wane some minutes before its final extinction. I got my last reliable glimpse of it at $8^h\ 52^m\ 16^s{\cdot}6$ L.M.T., which I assume to be $= 8^h\ 51^m\ 59^s{\cdot}1$ G.M.T. Its disappearance, according to the *Nautical Almanac*, should have taken place at $8^h\ 50^m\ 40^s{\cdot}4$ G.M.T.; showing an error of $1^m\ 18^s{\cdot}7$. I confess, however, my ignorance as to whether the final extinction of a satellite, the instant when it is theoretically dichotomized,—or what?—is spoken of as an eclipse, in our National Ephemeris.

Occultation of the Planet Vesta by the Moon on Dec. 30, 1871. By Mr. Hind.

The planet *Vesta* will be visibly occulted by the Moon in these latitudes on the evening of December 30. Though the phenomenon is one merely of astronomical curiosity, I have calculated the circumstances for Greenwich, and find them as follow:—

Immersion	Dec. 30	$10^h\ 44^m$ M.T.	88°
Emersion	,,	11 52	241

I am not aware that there is upon record any observed occultation of this planet, or of any other member of the group. The brightness of *Vesta* will render the observation an easy one, in good telescopes.

Observations of Encke's Comet. By H. W. Hollis, Esq.

I note that, at the last meeting, certain remarks were made by the Astronomer Royal and others upon the appearance of Encke's Comet. I have paid considerable attention to this object by means of my 8-inch achromatic, and as I have not yet seen any report which agrees with my own observations, I beg to lay before the Society a drawing copied from one made by myself at the telescope on the evening of the 12th November last, with a Kellner eye-piece, power 120. The drawing is carefully made, and I consider truly represents what I saw. My attention was particularly arrested by the sharp definition of the two edges of the fan-shaped nebulosity; and I could not help asking myself whether the true form of the body were not in all probability a hollow cone. I say a *hollow* cone, because I distinctly saw by averted vision a slight condensation of the light towards both edges, particularly the one running N. and S., on the following, or eastern, side of the comet, whereas a solid cone would appear brightest in the middle and grow fainter to the edges. I made an attempt to

measure with the position-micrometer the angle formed by the two well-defined edges, but the comet would not bear any illumination of the wires, so I inserted a thick wire (made of a horse-hair) which I could see plainly against the sky, and by this I made the angle 86 degrees ±. Definition was exceptionally good, the next entry in my observatory-book being "δ *Cygni* clearly split."

Keele, Newcastle, Staffordshire, Dec. 2, 1871.

Observations of Tempel's Comet (1871, *Nov.* 3), *at Mr. Bishop's Observatory, Twickenham.* By J. R. Hind, F.R.S.

	Twickenham M.T.	R.A.	N.P.D.
	h m s	h m s	° ′ ″
Nov. 8	5 47 34	18 39 50·07	104 9 15·9
9	6 9 24	18 40 17·80	105 7 40·8
10	5 51 29	18 40 43·95	106 4 19·1
12	5 52 38	18 41 38·70	107 57 24·7

Post-perihelion Places of Tempel's Comet, calculated from Mr. Hind's Elements. By Mr. W. E. Plummer.

0^h G.M.T.	R.A.	N.P.D.	Log. Δ.
	° ′	° ′	
1871, Dec. 21	281 50	139 55	0·1340
29	280 6	145 30	0·1142
1872, Jan. 6	277 23	151 21	0·0786
14	273 2	157 20	0·0342
22	264 14	164 8	9·9795
30	233 30	171 28	9·9171
Feb. 7	151 8	168 55	9·8546
15	126 16	153 7	9·8108
23	119 40	133 24	9·8105
Mar. 2	117 22	115 1	9·8597
10	116 46	100 59	9·9370
18	117 4	91 11	0·0199

On Feb. 15 the intensity of light is three times that at the date of the Comet's discovery, and on March 10 it is precisely the same. The Comet may be again observed in Europe in March.

Elements and Ephemeris of Tempel's Comet. By J. R. Hind, F.R.S.

The following elements are founded upon an observation by Dr. Winnecke at Carlsruhe on Nov. 5, and two observations at Mr. Bishop's Observatory on Nov. 8 and 10.

Perihelion Passage, 1871, Dec. 20·29115 G.M.T.

Longitude of Perihelion	263° 33′ 52″	} Apparent Equinox, Nov. 0.
,, Ascending Node..	147 28 38	
,, Inclination	82 31 21	

Log. Perihelion Distance 9·8321610.

Motion retrograde.

For the middle observation $\Delta\alpha\ (c-o) = +7''$, $\Delta\alpha . \cos . \delta = +11''$.

Hence the following Ephemeris, which may be useful in the reduction of the observations :—

0^h G.M.T.	R.A. h m s	N.P.D. ° ′	Log. Δ.
Nov. 3	18 37 44	98 57·3	0·10155
4	38 7	99 58·0	0·10355
5	38 30	100 58·0	0·10516
6	38 54	101 57·6	0·10698
7	39 19	102 56·6	0·10881
8	39 45	103 55·1	0·11066
9	40 11	104 53·0	0·11253
10	40 38	105 50·5	0·11438
11	41 5	106 47·4	0·11621
12	41 32	107 43·8	0·11803
13	42 0	108 39·7	0·11985
14	42 28	100 35·1	0·12167
15	18 42 57	110 29·9	0·12349

The co-ordinate constants, for the equators, are,

$$x = r . [9{\cdot}9274093] . \sin (v + 21^\circ\ 20'\ 37'')$$
$$y = r . [9{\cdot}7590534] . \sin (v + 175\ 17\ 16)$$
$$z = r . [9{\cdot}9898798] . \sin (v + 283\ 26\ 2)$$

Ephemeris of Tuttle's Comet for the Southern Hemisphere. By J. R. Hind, F.R.S.

The following Ephemeris is calculated from the elements of Dr. Tischler, but with perihelion passage taken Dec. 1·7974 G.M.T. When found by Dr. Winnecke on October 14, and considered pretty bright, I = 0·58.

0^h G.M.T.	R.A. h m s	N.P.D. ° ′	Log. Δ.	I.
1871, Dec. 18	11 52 2	143 5·5	9·9286	1·24
20	11 58 57	145 20·6	9·9399	
22	12 6 12	147 27·2	9·9511	1·08
24	12 13 48	149 25·7	9·9623	
26	12 21 45	151 16·3	9·9734	0·94
28	12 30 3	152 59·5	9·9843	
30	12 38 42	154 35·6	9·9950	c·82

0h G.M.T.	R.A.	N.P.D.	Log. Δ.	L
	h m s	° '		
1872, Jan. 1	12 47 44	156 5·0	0·0054	
3	12 57 9	157 28·0	0·0156	0·72
5	13 6 58	158 45·1	0·0255	
7	13 17 12	159 56·4	0·0349	0·63
9	13 27 50	161 2·3	0·0441	
11	13 38 52	162 3·0	0·0530	0·55
13	13 50 18	162 58·7	0·0616	
15	14 2 7	163 49·8	0·0698	0·48
17	14 14 16	164 36·7	0·0778	
19	14 26 43	165 19·5	0·0854	0·43

Elements of Minor Planet (116). By Dr. R. Luther.

Epoch 1871, September 23h·0 Berlin Mean Time.

$$L \;..\; 8^\circ\, 58'\, 12''\!\cdot\!6$$
$$M \;..\; 221\;\; 44\;\; 19\cdot7$$
$$\left.\begin{array}{l} \varpi \;..\; 147\;\; 13\;\; 52\cdot9 \\ \Omega \;..\; 64\;\; 14\;\; 13\cdot0 \\ \iota \;..\; 3\;\; 35\;\; 28\cdot7 \end{array}\right\}\text{Mean Equinox, } 1871\cdot0.$$
$$\varphi \;..\; 8\;\; 31\;\; 35\cdot0$$
$$\mu \;..\; 764''\cdot812143$$
$$\text{Log. } a \quad 0\cdot4443012$$

These elements have been computed from observations made in Germany.

Comet IV., 1871, discovered by M. Tempel at Milan on the 3d November.

The elements, calculated by MM. Oppolzer and Schulhof, are as follows:—

$$T = \text{December } 20\cdot1155 \text{ Berlin M.T.}$$
$$\left.\begin{array}{l} \varpi = 22^\circ\, 25'\, 39'' \\ \Omega = 145\;\; 19\;\; 53 \end{array}\right\}\text{Mean Equation } 1871\cdot0.$$
$$i = 102\;\; 7\;\; 40$$
$$\text{Log } q = 9\cdot87628.$$

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXII. *January* 12, 1872. No. 3.

WILLIAM LASSELL, Esq., F.R.S., President, in the Chair.

William Godward, Esq., 5 Clarence Road, Finsbury Park;
William Bath Kemshead, M.A., Ph.D., 53 Knowle Park, Brixton;
Frederick J. Marriott, Esq., Perry Hill, Sydenham;
Charles Frederick Plant, Esq., Sandfield House, Mount Vernon, Nottingham;
Chintamaney Ragoonathachary, Madras Observatory; and
George Kinneard Winter, Esq., Telegraph Engineer to the Madras Railway,

were balloted for and duly elected Fellows of the Society.

THE TOTAL SOLAR ECLIPSE OF 12 DEC. 1871.

Letter from M. Janssen to the President.

J'aurai l'honneur d'adresser à la Société Royale Astronomique un mémoire détaillé de mes observations de l'Eclipse, mais je profite du départ de ce courrier pour vous informer des principaux résultats obtenus.

Sans entrer dans une discussion qui fera partie de ma relation, je dirai d'abord que la magnifique couronne observée à Sholoor s'est montrée sous un aspect tel qu'il me paraîtrait impossible d'admettre ici une cause de l'ordre des phénomènes, ou de diffraction, ou de réflexion sur le globe lunaire, ou encore de simple illumination de l'atmosphère terrestre.

Mais les raisons qui militent en faveur d'une cause objective et circumsolaire prennent une force invincible quand on interroge les élémens lumineux du phénomène.

En effet, le spectre de la couronne s'est montré dans mon télescope, non pas continu comme on l'avait trouvé jusqu'ici, mais remarquablement complexe. J'y ai constaté :

Les raies brillantes, quoique bien plus faibles, du gaz hydrogène qui forme le principal élément des protubérances et de la Chromosphère ;

La raie brillante verte qui a déjà été signalée en 1869 et 1870, et quelques autres plus faibles ;

Des raies obscures du spectre solaire ordinaire, notamment celle du sodium (D.) Ces raies sont bien plus difficiles à apercevoir.

Ces faits prouvent l'existence de matière dans le voisinage du Soleil, matière qui se manifeste dans les éclipses totales par des phénomènes d'émission, d'absorption, et de polarisation.

Mais la discussion des faits nous conduit plus loin encore.

Outre la matière cosmique, indépendante du Soleil, qui doit exister dans le voisinage de cet astre, les observations démontrent l'existence d'une atmosphère étendue, excessivement rare, à base d'hydrogène, s'étendant beaucoup au de-là de la Chromosphère et des protubérances, et s'alimentant de la matière même de celles-ci, matière lancée avec tant de violence à travers la photosphère, ainsi que nous le constatons tous les jours.

La rareté de cette atmosphère, à une certaine distance de la Chromosphère, doit être excessive ; son existence n'est donc point en désaccord avec les observations de quelques passages de comètes près du Soleil.

Neelgherry, Sholoor, 19 Décembre, 1871.

Letter from Col. Tennant to Dr. Huggins.

At 8 A.M. I sent you the following telegram :—

"Thin mist, spectroscope satisfactory. Reversion of lines entirely confirmed. Six good photographs."

I have now just come down the hill, leaving my able assistants to dismantle ; and I hasten to give you a sketch of what has been done, to go by to-night's mail.

At $19^h\ 31^m\ 50^s$ I saw a projection from the Moon's cusps, and on this set the slit of the spectroscope. Herschel saw *many lines.* He wished to note, but by a misapprehension I did not give him the signal till some twenty seconds later, when he got a few from the rapidly moving cusp which I was following. This ended at the apparent top of the Moon by the slit passing through a prominence.

The lines appeared rapidly one after another, and I believe it would have been impossible to record them or to identify more than one or two previously marked with pointers.

I then, by agreement, came to the centre of the Moon, which was reported blank. We placed the slit across the edge of rift whose position (that of the edge) I estimated at 120° from the vertex. It was about 10′ from the Moon, and K 1474 was, I believe, the only result in lines, and it seemed to cross the field; but to this point sufficient attention unfortunately was not paid, from my having omitted to tell Herschel what he was looking at. I then turned to a thin rift truly above the Sun, and some 8′ away from the Moon. K 1474 was again only the result, though in both cases Herschel seems to have seen *traces* of lines. I then about doubled the distance from the Moon. I had still light, and saw the rift; but Herschel had no lines. Finally, I approached the rapidly coming edge of the Sun, and lines began to come quickly, but the Sun was out before any could be recorded.

I have only had a conversation with Herschel, and he cannot see this before it goes and correct it, so that I may be wrong in some detail, but, I believe, this is what he reported.

For myself I was using the Directing Telescope on what we have unanimously called the Twins. I was intent on following the cusps for Herschel, and thus lost the opportunity of examining the polarization of the light which first threw out the whole Moon. I had my attention specially directed to the chromosphere and prominences at the true bottom of the Moon's limb. They first appeared *white*, and then changed through pink to red. I am quite confident that I saw no blue or green tinge. You will remember my suspicion that I do not readily see blue when faint, but I am sure the tint was not strong. My waiting in the Moon's centre gave me an opportunity of looking about me, and I saw the two rifts I have spoken of. To me they did not reach the Moon's edge, but were separated by some minutes of bright corona. The corona's outer layers were undoubtedly radiated, but not coloured. There were alternate gradations of light; that is, light and comparative darkness, but certainly no colour. The rays were lost as they neared the Moon. The rift at the true vertex of the Sun I am sure did not change, nor I believe did any other. Colonel Saxton had made for my object-glass a loose cap of pasteboard, with a two and a half inch aperture to be used during the Sun's presence; this was to have been knocked off just before totality. I forgot this, but I doubt if I should have improved matters; I used the power 35.

I had asked Captain Morant, R.E. (to whom I am indebted for a very great amount of assistance), to take a very beautiful reconnoitring telescope by Dallmeyer, which was mounted on a stand. Of 14 inches focus, it has also power (15) and an aperture of 1·75 inches. He (Captain Morant) made two sketches of the corona, which generally confirm, as does his description, all I have described. He expressed himself positive as to the absence of colour and the permanence of rifts, &c., in certain positions (not mine), and only differed in thinking that the shades

were of a sepia tinge (brownish) and that the rifts did extend to the Moon.

Now to the photographs. We have had some glorious mornings. Yesterday Herschel and I were up at a *Argûs*, but had no great amount of examination, as our intention was to focus slit, &c. on stars. Though the evenings have been misty, at times the mornings have been very clear, and we hoped they would remain so. Last night was very clear, and continued so till about $2^h 30^m$ a.m., when it became cloudy, and afterwards rained slightly. By the time for getting up it was less thick, and rain had ceased, but a wind had sprung up from the S.E. (I believe), which, of course, kept a driving mist on the hill. The Sun seemed to lose as much power by obscuration as it gained by rising, and was powerless to disperse this, as it would otherwise have done, and our instruments were *dripping*. This I had thought possible in such a case, but I had hoped that the fine weather had set in, as such mist seems abnormal so late in the season. Very few minutes after totality the mist vanished from the returning heat.

Under these circumstances one might have thought photographs hopeless, and I went despondingly to ask for results. I found, to my great astonishment, that they had six apparently good ones, which is due to Hennessy, who exposed, having thrown over our programme, and increased his exposures, while Captain Waterhouse, who did the photographer's part ably, backed him by rapidly changing the slides.

A more careful examination than I could make before my telegram left, showed me that one of the photographs (the sixth) was imperfect, and, as luck would have it, this met with an injury by the removal of a portion of the film in the corona. The others are good. In all the Moon is elongated (from motion ?), but the rifts and radiated structure of the corona are clear, and the unchangeability seems, by a rapid examination, confirmed. For more than this I have not had time, as we got breakfast early, in hopes of getting all the instruments down-hill to-day, and I left soon after to my letters.

Herschel fitted up the Royal Society's 5-inch (in a way which he will, doubtless, describe) as an integrator, and instructed Col. Saxton in its use. I believe the reversion of the main lines was seen with it, but during totality only K 1474.

Ootacamund, Dec. 12, 1871.

Dr. Huggins has also received from Mr. Nursing Row, Fellow of the Society, a letter dated Dara Gardens, Vizagapatam, 13th Dec. 1871, containing an account of his observations of the Eclipse. The position of the Observatory is Lat. 17° 42′ 9″ N., Long. 83° 22′ 30″ E.; the computed times were—

	h	m	s
Beginning	7	0	24
Greatest Phase . . .	8	1	36
End	9	6	2

the magnitude being ·629. The Eclipse was well seen, not a cloud in the vicinity of the Sun, and according to the observations the errors of the predictions were, for the beginning −12 seconds, for the greatest phase + 10 seconds, and for the end + 33 seconds.

After the greatest phase, the edge of the Moon's limb on the Sun's disk was observed, somewhat rough and uneven. At about six minutes to the end of the Eclipse this uneven edge attained a tinge of yellow colour, undulating. At the greatest phase the light of the Sun was dull, and on grassy grounds looked pale yellow.

A mean of the meteorological observations was forwarded, containing observations of the solar radiation and the humidity for every ten mimutes. The radiation observations were made with the solar-radiation thermometer, exposed freely to the Sun's rays in an open grassy place at the height of one foot from the ground, resting on two pieces of forked stick. The instrument was graduated on the stem, and had no attached scale, but enclosed in a large tube with a large bulb at one end, in the centre of which was the thermometer-bulb covered with lamp-black. The large tube was expanded with air and hermetically sealed.

There was also inclosed the copy of a sketch of the Sun's image previous to the beginning of the Eclipse, showing the positions of the spots then visible.

Meteorological Observations taken during the Time of the Eclipse, 12th December, 1871.

Hours. h m	Barometer.	Dry Bulb.	Wet Bulb.	Humidity.	Solar Radiation Thermometer.	Kind of Clouds.	Clear Sky.
7 1	30·138	73·5	67	69	40	ci	80
7 11	30·138	73·8	67	67.	42	ci	85
7 21	30·142	74·3	67·5	68	46	ci	95
7 31	30·148	75·3	68·5	68	50·5	ci	95
7 41	30·154	75·6	68·5	68	50·5	ci	95
7 51	30·156	75·7	68·5	68	49	ci	95
8 1	30·160	75·8	68·7	68	48	ci	99
8 11	30·166	76	68·8	68	49	ci	90
8 21	30·170	76	68·8	68	51	ci	90
8 31	30·174	76·2	69	68	62	ci	90
8 41	30·180	76·5	69·1	68	68	ci	90
8 51	30·184	77	69·5	67	77	ci	90
9 1	30·192	77·2	69·8	68	82	ci	90
9 6	30·196	77·5	70	67	86	ci	88

On the Expression of M. Delaunay's $h+g$ in terms of his finally adopted Constants. By Prof. Cayley.

I had the pleasure of receiving from M. Delaunay a letter dated Paris, 17th Dec. 1871, in which he informs me that, on referring to his papers, he had found there expressions for l, g, h, identical with those given by me in the November Number of the *Monthly Notices*,— with only a single typographical error, $\frac{23}{33}e'^2m^3$ instead of $\frac{23}{32}e'^2m^3$ in my expression of h.

M. Delaunay mentions also that he had obtained four additional terms in the expression for $h+g$ (longitude of the Moon's perigee), and that the complete expression in terms of the finally adopted constants is —

$$h+g=$$

$$\begin{aligned} nt\Big\{&\left(\frac{3}{4}-6\gamma^2-\frac{3}{8}e^2+\frac{9}{8}e'^2-\frac{45}{4}\gamma^4+\frac{69}{8}\gamma^2e^2-9\gamma^2e'^2-\frac{3}{32}e^4-\frac{9}{16}e^2e'^2+\frac{45}{32}e'^4\right)m^2\\ &+\left(\frac{225}{32}-\frac{189}{8}\gamma^2-\frac{675}{64}e^2+\frac{825}{32}e'^2+\frac{1107}{16}\gamma^4+\frac{81}{32}\gamma^2e^2-\frac{349}{4}\gamma^2e'^2-\frac{2475}{64}e^2e'^2\right)m^3\\ &+\left(\frac{4071}{128}-\frac{3963}{32}\gamma^2-\frac{31605}{512}e^2+\frac{61179}{256}e'^2\right)m^4\\ &+\left(\frac{265493}{2048}-\frac{335403}{512}\gamma^2-\frac{1483665}{4096}e^2+\frac{1767849}{1024}e'^2\right)m^5\\ &+\left(\frac{12822631}{24576}-\frac{25291729}{16384}e^2\right)m^6\\ &+\left(\frac{1273925965}{589824}+\frac{352038855}{1179648}e^2\right)m^7\\ &+\frac{71028685589}{7077888}m^8\\ &+\frac{32145914707741}{679477248}m^9\\ &+\left[\frac{45}{32}m^2+\frac{7425}{512}m^3\right]\frac{a^2}{a'^2}\Big\}\end{aligned}$$

On the Zodiacal Light. By Captain Tupman.

During a stay of some three years in the Mediterranean I had many opportunities of observing the Zodiacal Light. Occasionally, but very seldom, it was so bright and its boundaries so well defined, it was easy to trace the latter on a star-chart with considerable accuracy.

From the beginning these observations appeared to me to

present features of great interest, and I endeavoured to continue them on every favourable opportunity, especially in the early mornings when the eye-sight is most sensitive. After December, 1870, no such opportunity occurred, the planets *Venus* and *Jupiter* remaining for months in the middle of it, just when the ecliptic was most favourably elevated above the horizon.

The variation of its light is very remarkable. Sometimes it attains a brilliancy that is not sensibly affected by the brighter planets shining in the midst of it, and after an interval of a few days its light may be quite faint.

Its apparent angular extension from the Sun doubtless depends upon the keenness of the observer's sight, the clearness of the atmosphere, and the general inclination of its axis of symmetry to the horizon. An illustrious Continental astronomer once assured me that he frequently saw it completing the circle of the heavens, with a small cloud-like maximum of brilliancy opposite the Sun; and there are the well-known observations of M. Liais in corroboration.

The sight of a particular observer, however, may be considered constant, and, hence, great actual variations of the intensity of the light, not only near the Sun, but in every part of its zone, must be admitted.

Another, and perhaps more singular, feature is the variable position of the apparent axis of symmetry with regard to the plane of the ecliptic. It is true that this element cannot be determined with accuracy, but there are times when an error of 3 or 4 degrees is inadmissible. Such times have been chosen for making the sketches I now send, which show, not only an inclination of the axis to the ecliptic, amounting, in the months of August and September, to 20°, but also that *its plane does not pass through the Sun.* Six of the morning observations agree in placing the eastern node 40° behind the Sun, and the only evening observation places the western node 42° before the Sun; in the former the axis crossing from North to South, in the latter from South to North. The observation on the morning of the 7th of January, 1870, places the axis parallel to and 3° North of the ecliptic, and on that occasion an error of 3° in its absolute position was highly improbable, as 3° of azimuth on the horizon appears to cover a large space.

The effect of refraction in raising the lower part of the more inclined border more than the upper part is quite inappreciable.

It would be exceedingly interesting to compare observations of this nature made, simultaneously or nearly so, at considerable distances on either side of the equator.

In conclusion, I have only to remark that the drawings were made on charts containing the stars only; the ecliptic was then added and the whole traced off. I send the original tracings.*

* These tracings were exhibited at the November Meeting, and are preserved for inspection in the *Library* of the Society.

Details of the Observations.

Place.	Day (Astron.) 1869.	Time *before Sunrise.* h m	Inclination of Axis of Symmetry to the Ecliptic.	Distance of Node of Axis from the Sun.	Distance of Apex from the Sun, in Longitude.	Latitude of Apex.	Curvature of N. and S. Borders and general Angle between them.	Intensity of the Zodiacal Light.	State of the Atmosphere.	No. of Sketch.	Notes.
Lisbon	Sept. 13	2 50	20°	☋ 42°	61°	−6°	Convex, 48°	v. v. bright	v. clear	1	The light fading off gradually at the borders. Axis almost coinciding with the parallel +18°.
,,	,,	1 50	20	☋ 42	51	−4	Convex, 38°	v. v. bright	v. clear	2	
,,	Sept. 14	2 50	20	☋ 42	61	−6	Convex, 48°	v. v. bright	v. clear	–	The sketches of yesterday exactly suit the appearances to-day.
,,	,,	1 50	20	☋ 42	51	−4	Convex, 38°	v. v. bright	v. clear	–	
Cadiz	Sept. 15	2 0	20?	—	60?	—	Convex	v. bright	v. clear	–	Appearance the same as on two previous days.
Gibraltar	Oct. 4	2 0	0	—	66	+2	straight, 35°	bright	v. v. clear	3	Axis certainly not inclined 3°. An hour earlier the apex was observed in the same place, but the cone was narrower.
,,	Oct. 8	2 10	5	♌ 56±8	69	+2	straight, 34°	bright	v. clear	4	Borders sharply defined (comparatively).
Malta	Oct. 11	4 0 to 3 0	0	—	81	+2	straight, or slightly concave, 43°	v. v. bright	v. clear	5	The apex stationary among the stars for more than two hours, and more sharply defined than usual. Axis sensibly coinciding with ecliptic.
,,	Oct. 14	3 0 to	0	—	84	—	—	bright	v. clear	–	Apex beyond the Præsepe in *Cancer*, but after the brighter portion rose, it could not be traced so far.

							well defined	display			
„	Nov. 3	1 30	0	—	69	—	Concave, ill defined, 26°	faint	v. clear	7	The light faint, although the air very clear.
„	Nov. 11	2 20	0	—	77	—	—	bright	v. clear	-	Apex extending nearly to *Regulus*.
Malta	1870. Jan. 7	3 10 to 2 10	0	—	113	+2	very concave	bright	v. clear	8	Axis parallel to, but 3° N. of ecliptic. At times the apex seen extending to *β Virg.* Base broad.
Vigo	Aug. 31	2 0	23	♑ 40	58	−6	Convex, 43°	bright	v. v. clear	9	The borders, if prolonged a little, would intersect at *γ Geminorum*.
„	Sept. 5	1 40	16	♑ 40	73	−7	slightly concave, 20°	exceedingly bright	v. v. clear	10	The light exceedingly bright between *Venus* and *Mars*, both planets being within its cone.
Malta	Dec. 14	2 15 after sunset	7	♌ 42	160 to 170	—	slightly concave	bright	v. clear	11	*Evening*. Easily traced nearly to *Pleiades*, and at times 10°–20° beyond.
„	1871. April 29	2 0 before sunrise	—	—	—	—	—	—	—	—	No trace of it, although the ecliptic most favourably situated.
„	May 7	1 40 after sunset	—	—	80	—	—	v. bright	v. clear	—	*Venus* and *Jupiter* both shining brightly in it. Axis about perpendicular to the horizon. Apex is in north latitude.
„	May 8	2^h to 3^h after sunset	—	—	100	+4	—	faint	clear	—	Much diffused. After *Venus* and *Jupiter* had set, seen extending to *α* and *γ Leonis*.
„	May 14	$1^h\,50^m$ after sunset.	—	—	93	+4	—	bright	clear	—	Extending to *α* and *γ Leonis*. *Venus* and *Jupiter* shining brightly in it.

Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, from 1871, January to December.

Communicated by the Astronomer Royal.

Occultations of Stars by the Moon.

Day of Observation.	Phenomenon.	Moon's Limb.	Mean Solar Time.	Observer.
1871.			h m s	
Jan. 31 (*a*)	Disapp. of *l* Tauri	Dark	4 48 34·5	J.C.
Mar. 3	Disapp. of η Cancri	Dark	11 9 18·4	H.C.
,,	Disapp. of 39 Cancri	Dark	15 1 13·2	,,
,, (*b*)	Disapp. of 40 Cancri	Dark	15 4 52·6	,,
Mar. 28 (*c*)	Reapp. of μ Geminorum	Bright	10 53 31·7	E.
Oct. 23	Disapp. of τ^1 Aquarii	Dark	8 41 16·6	L.
,,	Disapp. of τ^2 Aquarii	Dark	9 49 29·7	,,
,, (*d*)	Reapp. of τ^2 Aquarii	Bright	10 59 30·2	,,
Dec. 20	Disapp. of ν Piscium	Dark	12 49 32·1	J. C.

(*a*) Observed in daylight; the star was very faint, and the point of disappearance being very near the bright limb, the time noted may be a few seconds too soon.

(*b*) The star very faint; observation not satisfactory.

(*c*) The star was seen quite bright quite close to the limb.

(*d*) The time noted is probably correct to a second.

Phenomena of Jupiter's Satellites.

Day of Observation.	Satellite.	Phenomena.	Mean Solar Time of Obs.	Mean Solar Time from N.A.	Observer.
1871.			h m s	h m s	
Jan. 5 (*a*)	I	Ecl. reapp.	6 11 57·9	6 11 48·4	C.
12	I	Ecl. reapp.	8 8 10·0	8 7 5·6	W. C.
17	I	Ecl. reapp.	15 33 34·3	15 33 34·8	,,
19	III	Tr. ing. 1st contact	7 43 57·4		J.C.
,,	III	,, bisection	7 48 56·5	7 47 ...	,,
,,	III	,, last contact	7 54 55·6		,,
26	I	Ecl. reapp.	11 57 49·3	11 58 1·8	C.
Feb. 6	III	Occ. reapp. 1st app.	7 8 7·9		J C.
,,	III	,, bisection	7 12 37·1	7 16 ...	,,
,,	III	,, last cont.	7 17 6·4		,,
13	III	Occ. disapp. bisection	8 18 47·8	8 20 ...	E.
Mar. 10 (*b*)	III	Tr. ing. 1st contact	9 59 22·3	10 0 ...	C.
22 (*c*)	I	Ecl. reapp.	8 55 32·7	8 55 14·3	,,
April 15	III	Tr. egr. last cont.	9 44 48·9	9 41 ...	E.
Sept. 12	IV	Ecl. reapp	14 32 44·5	14 39 20·2	J.C.
Oct. 13	II	Ecl. disapp.	12 32 56·1	12 31 23·4	G.
22 (*d*)	III	Ecl. reapp.	12 11 51·6	12 12 28·4	W. C.

Day of Observation.	Satellite.	Phenomena.	Mean Solar Time of Obs.	Mean Solar Time from N.A.	Observer.
1871.			h m s	h m s	
Nov. 5	III	Ecl. disapp.	17 0 49·1	16 58 29·9	G.
18 (*e*)	IV	Ecl. reapp.	15 6 23·2	15 12 27·0	W. C.
Dec. 4 (*f*)	III	Ecl. reapp.	12 7 55·8	12 5 47·5	G.
,,	III	Occ. disapp. 1st cont.	12 25 52·8	12 30 ...	,,
,,	III	,, last cont.	12 29 7·3		,,
29	III	Tr. ing. 1st cont.	12 16 36·4	12 19 ...	C.
,,	III	,, last cont.	12 23 5·3		,,
30	I	Ecl. disapp.	10 47 38·9	10 48 11·5	G.
,,	IV	Tr. ing. 1st cont.	11 7 45·6	11 21 ...	,,
,,	IV	,, last cont.	11 12 14·8		,,
,,	IV	Tr. egr. bisection	15 25 33·2	15 35 ...	E.
,,	IV	,, last cont.	15 32 32·1		,,

(*a*) Clouds passing over the planet.
(*b*) Cloudy; observation not satisfactory.
(*c*) The images of *Jupiter* and the satellites were very tremulous and faint.
(*d*) The time noted is very accurate. The satellite was of extreme faintness when first noticed.
(*e*) The satellite when first seen was extremely faint, and certainly was not visible 10ˢ before.
(*f*) Satellite very faint; it reappeared near the planet.

The initials W. C., E., C., L., J. C., H. C., and G., are those of Mr. Christie, Mr. Ellis, Mr. Criswick, Mr. Lynn, Mr. Carpenter, Mr. H. J. Carpenter, and Mr. Goldney.

An Early Transit of Mercury. Communicated by the Rev. A. Freeman, M.A., Fellow of St. John's College, Cambridge.

A transit of *Mercury* was observed at St. John's College, Cambridge, in 1782. The MS. record, never yet published, still exists. It appears to be in the handwriting of Dr. Isaac Pennington.

"Nov. 12, 1782.

	m s	
Regulator Clock slow	6 : 4·5	
Equation of Time	15 : 32 ‡	
Apparent Noon	XIʰ : 38 : 22·5 per clock.	
☿ Ingress	II : 51 :	apparent time.
Ingress	II : 29 :	per clock.
☿ Egress	IV : 15 :	apparent time.
Egress	III : 53 :	per clock."

This observation is the more valuable, because clouds prevented Dr. Nevil Maskelyne from completing the observation at Greenwich. In the *Greenwich Observations* for 1782, he remarks, that, "On the going off of a cloud I first saw *Mercury* with the 46-inch

achromatic, magnifying 200 times, to make a considerable notch in the Sun's limb at $2^h\ 49^m\ 53^s$ apparent time, and at $2^h\ 54^m\ 55^s$ apparent time, I first was certain of light appearing between the limbs of the Sun and *Mercury*."

The mean of Maskelyne's observations would give a time $2^h\ 52^m\ 24^s$, which would be somewhat later than the ingress of *Mercury's* centre. Allowing 24^s for east longitude of Cambridge, this differs by 1^m from the record of St. John's College. The duration of the transit was $1^h\ 24^m$, remarkably long.

It should be noted at (‡) that the *Connaissance des Temps pour l'an* 1782 gives as the Equation of Time for the day $15^m\ 33^s$. This number of *seconds* is evidently required to make the sum of the first three times of the above record exactly equal to XII^h.

An account of the old Observatory of St. John's College, removed in 1859, will be found in the observations of the Rev. Thomas Catton, made between 1791 and 1826, and published in Vol. xxii. of the *Memoirs of the Royal Astronomical Society*, 1854.

Proposed devotion of an Observatory to observation of the phenomena of Jupiter's Satellites. By G. B. Airy, Astronomer Royal.

The position which the Royal Astronomical Society holds in the astronomical world may well justify it in employing its judgment and its influence in the direction of astronomical enterprises exterior to its own body. Guided by this consideration, I venture to submit to the Society whether they may not adopt, as an idea worthy of promulgation under their auspices, the proposal that one Observatory should be permanently devoted to the observation of the phenomena of *Jupiter's* satellites.

It is well known to the students of Gravitational Astronomy that the theory of the movements of *Jupiter's* satellites is a very singular one, perhaps the most interesting among the planetary applications of the theory of Gravitation. And the results are striking, especially in the remarkable enchainment which they exhibit in the movements of the three interior satellites, which at the same time are effected by the mass of the fourth. The fourth satellite has a claim peculiar to itself; it is on that satellite that observations must be made for determining the mass of *Jupiter* (the element which in the solar system is next in importance to the mass of the Sun). It is by it that the mass of *Jupiter* was determined originally (with some inaccuracy) by Pound, and subsequently in the first instance by myself (as is detailed in the *Memoirs* of this Society), and in the second instance by Bessel.

In late years the observations of the satellites have been

greatly neglected. The few observations that have been made of the third and fourth satellites are sufficient to show that the errors of their tables are too large to enable us practically to use these objects for even rude time-determinations. and at the same time are far too rarely determined to enable us to correct the elements of the tables. Of the first satellite there are, perhaps, sufficient observations, possibly also of the second satellite. But it must always be borne in mind that the tabular movements of the satellites cannot be corrected one by one; the necessary relation among their motions, to which I have already alluded, compels us to treat all together, and in combination with one of the most delicate theories of science.

The reason of the neglect to which I have alluded appears to be this, that the observations of such phenomena (occurring at what may be called irregular times, but times which are perfectly peremptory when the phenomena do occur) interfere most injuriously with the good order of the one class of observatories which has been strictly brought to rule, namely, the meridional observatories. And I see no prospect of obtaining a sufficient number of observations unless some observer would consent to give his entire observing energies to these observations. The accompanying observations for correction of clock would be of so light a character that I do not think them worthy of further notice.

It may be well to remark that, even to an observer who should determine to neglect no observation, a very fair amount of holiday would be left. Thus in 1872 there is no eclipse of satisfactory character visible at Greenwich between May 18 and September 18, and no other phenomena between June 9 and August 31. The interval between observations of less satisfactory character is from July 3 to August 29. In the winter oppositions of *Jupiter* the observations of satellites are rather pressing, for a time, but much less so in the summer oppositions.

The phenomena which first call for attention are undoubtedly the eclipses. Next to these, I am enabled to say on the authority of Laplace, are the transits of shadows upon the planet's disk, if they can be observed with sufficient accuracy. They possess this remarkable merit, that they are computed by the very same tables with which the eclipses are computed, without reference to the position of the Earth. Possibly the observation of transits of the satellites over the disk, and of occultations behind the disk, would be more accurate; they require, however, an additional calculation, depending on the position of the Earth. It would, however, be very desirable to examine all.

Having myself, in past years, repeatedly observed the phenomena of every class, and having at a more distant time studied the mechanical theory applicable to them, I may venture to express my belief that the more observations, in their beauty and the incessant variation of their character, would be found very interesting; but that the highest interest would be felt on connecting them with their appropriate theory, and in making the

preparations for that numerical work by which each observation would be made to bear its part in the correction of elements and in the ultimate test of the theory.

Royal Observatory, Greenwich,
1871, December 26.

Remarks on the Planet Jupiter. By W. Lassell, Esq.

I send a sketch and a few remarks upon the planet *Jupiter*.

Now that the fourth satellite has begun again for a season to cross the planet's disk, I have looked out for opportunities of observing its transit, and was favoured on the night of the

30th Dec. last by witnessing a part of its passage over the disk under circumstances more than usually propitious.

On its first entrance it was scarcely to be distinguished from the limb, not appearing at all, as the others generally do, as a round, bright spot. As it advanced it grew gradually manifestly darker than the surface of the planet, and by the time it had [illegible] a fourth of the way across, it had become a very dark [illegible] spot,—so dark indeed that if I had looked at

Jupiter without knowing any thing of the positions of his satellites, I should have said that a *shadow* was transiting. I remember having seen the like phenomenon many years ago; but my impression is that I had never seen the disk of the satellite so near to absolute blackness before. Of course it is only by contrast that it can possibly so appear; and we have in this fact a striking proof of the exceeding brilliancy of the surface of the planet. In the same way the solar spots, if not surrounded by the marvellous splendour of the Sun's surface, would doubtless appear as brilliant objects.

But this was not the phenomenon which struck me most in this rare and exquisite view of *Jupiter*. I acknowledge that I have hitherto been inclined to think that there might be some exaggeration in the *coloured* views I have lately seen of the planet; but this property of the disk in the view I am describing was so unmistakable, that my scepticism is at least beginning to yield.

I have attempted in the accompanying drawing, to represent the colours as faithfully as I can, and to convey something like a general notion of the distribution and intensity of the various lights and shades scattered over the planet; but to give any thing like a faithful outline of the individual phenomena so beautifully displayed on this occasion, is far more than I can pretend to. It is one of the advantages of a Newtonian reflector, when the alloy of the specula is well compounded, that colours of the planets are more faithfully represented than they can be by refracting telescopes, the want of perfect achromatism in the latter generally introducing some modifying tinge.

The picture is the result of the use of various powers,—260, 430, and 579, the last too high for the state of atmosphere,—and it was a doubtful point which of the others was to be preferred, though there were tranquil moments of the atmosphere when I thought 430 revealed the most. The aperture was 24 inches, which indeed I rarely diminish.

I may add further that the variety of colour is most obvious with the higher powers, and when the atmosphere will not permit their use the tints are scarcely apparent.

As the coloured drawing may present some difficulty of reproduction I inclose also a pencil drawing, with the names of the tints or colours opposite the respective parts, which may in some degree supply the place of the coloured drawing.

Ray Lodge, Maidenhead,
Jan. 2, 1872.

On the Probable Seat of Energy of the Eruption Prominences.
By A. C. Ranyard, Esq.

The enormous rate of ascent occasionally observed in the upheaval of eruption prominences—a most striking instance of which has been carefully discussed by Mr. Proctor in the last number of the *Monthly Notices*—leads us to inquire whether such vast velocities do not betoken something more than mere vertical storms in a gaseous envelope; or why the rate of motion observed in vertical storms or outrushes so greatly exceeds the velocities detected by the spectroscope in the horizontal currents. Further, have we any ground for conjecturing that the prominences are driven upwards by explosions taking place at great depths; or that they are carried outwards by solid masses hurled through the solar envelopes driving before them and followed after in their train by vast eddies of the vapours through which they pass?

When we consider the enormous temperature of the solar envelopes in contact with the photosphere, (estimated even by Zöllner at 68400° C., and calculated by Ericsson at 4035584° Fahr., and by Secchi at over ten million degrees Centigrade), it is at first difficult to conceive of matter existing in the Sun in any other state than that of vapour.

We are, however, so little acquainted with the behaviour of the elements under pressures and temperatures comparable with those which must exist at great depths within the solar globe, that it would be impossible to predict that there is no solid core to the Sun.* Prof. Young, speaking of the great prominence he observed in September last, says, "The whole phenomenon suggested most forcibly the idea of an explosion under the great prominence, acting mainly upwards, but also in all directions outwards." It is not necessary to conceive of chemical change in order that an explosion may take place; some alteration in the physical condition, some passage from the liquid to the gaseous state caused by an alteration of pressure—and involving no chemical change, may be sufficient to cause such an explosion, which, if it took place causing an expansion of the lower and denser vapours, would force them upwards into the outer and lighter envelopes, where, by reason of their greater specific gravity they would be carried further in the resisting medium than the lighter vapours composing the layer above them. Under such a supposition we might expect to find in the newly formed prominences their head or upper part composed of the denser vapours, and the lighter gases lagging behind. This, however, is the reverse of what is found upon the Sun. The prominences have their summits entirely composed of hydrogen, while they are often "gonflé"

* In other words, we cannot predict that the critical temperatures of some of the elements may not be even greater than solar temperature. See Prof. James Thompson's Paper before the British Association, Edinburgh.

with sodium and metallic vapours which have apparently been forced up into them from below, giving a structure such as we might expect to find if a series of layers had been disturbed by matter passing through them raising them up into prominences one inside the other.

The spiral twists which exist in the tall finger-shaped prominences do not form any insuperable objection to this theory of their eruption, for they may be caused after a very short lapse of time by horizontal vortices existing in the non-incandescent medium into which the prominences are projected.

It may be said that the existence of metallic vapours higher up than they are detected by the spectroscope is by no means improbable, and that because they have ceased to give out monochromatic light it does not follow that they are not there. To this it may be answered, that if such metallic vapours are there, and if they are at the same heat as the intensely incandescent hydrogen which forms the prominences, or, indeed, at the heat of a region of space so near to the photosphere, they would certainly be in a state of vapour; and if in a state of vapour, why not incandescent with their own wave length?

A theory has lately been started that meteors may be formed from the metallic vapours hurled from the Sun or other stars in their eruption prominences.

It appears, however, from the analysis of the Lenarto meteor given by the late Prof. Graham, and quoted by Mr. Proctor in his recent Paper on the "Corona," that the iron of which the meteor was chiefly composed contained at least three volumes of occluded hydrogen, pointing, as Prof. Graham believed, to its formation under great pressure in a dense mass of hydrogen such as we find in the chromosphere. Probably, however, there are no pressures in the chromosphere at all comparable even to atmospheric pressures, we are therefore forced to look to deeper levels (if we look to the Sun at all) as the laboratory where meteoric iron becomes solid, and to believe that the meteors were extruded from the Sun in a solid form* containing the hydrogen they at present hold occluded in them. These considerations taken in conjunction with the remarkable flashes across the chromospheric spectrum observed by Zöllner, Vogel, and Howlett in this country, appear to me to go far to render it probable that solid masses may be hurled from the Sun in eruption prominences.

* On this supposition meteorites would have very high, perhaps infinite, critical temperatures, an assumption surely less improbable than that we are forced to make on the theory that hydrogen is the exploding element, giving rise by its outrush to the prominences, in which case we must assume for it a critical temperature,—at least higher than the minimum solar temperature,—whereas hydrogen has not yet been reduced by pressure to the liquid state even at the lowest terrestrial temperatures. Of the twenty-two meteoric elements given by Miller, there are, however, others besides hydrogen which have not yet been reduced separately.

Description of a Printing Chronograph.

By G. W. Hough, Director of the Dudley Observatory.

About the year 1848 the idea of recording astronomical observations by the use of galvanic electricity was put in successful operation by different individuals. Since that time chronographs of various forms have been constructed for recording in a legible manner, on a moving sheet of paper, the time of any phenomenon observed. The great superiority, in point of accuracy and saving of labour over the old eye-and-ear method formerly used, led to the almost general adoption of the new plan.

During the past ten years the idea of constructing a chronograph which should print with type the time of the observation has been entertained by a number of persons. About five years since Prof. Hilgard, of the U.S. Coast Survey, made a description of an apparatus designed for this purpose, and about the same time Prof. C. A. Young, of Dartmouth College, published a proposed plan for one in *Silliman's Journal of Science.* But, so far as we are informed, the mechanical construction of such an apparatus has not heretofore been attempted by any one.

The construction of a machine which shall carry a type-wheel capable of given impressions, with uniform velocity for a number of hours together, without sensible variation in its motion, is a problem which is not easy of solution.

For a clear understanding of the mechanism elaborate drawings would be necessary, we shall therefore merely give a general account of its construction and peculiarities:—

1st. A system of clock-work carrying a type-wheel with fifty numbers on its rim, revolving once every second; one, two, or parts of two numbers being always printed, so that hundredths of seconds may be indicated. This train is primarily regulated to move uniformly by the Frauenhofer friction-balls, and secondarily by an electro-magnet acting on the fast-moving type-wheel, and controlled by the standard clock. This train is entirely independent, and can be stopped at pleasure without interfering with the other type-wheels.

2nd. A system of clock-work, consisting of two or more shafts, carrying the type-wheels indicating the minutes and seconds. The motion of this train is also governed by an electro-magnet, controlled by the standard clock, operating on an escapement in a manner analogous to the action of an ordinary clock, every motion of the escapement advancing the type one number.

There are three type-wheels, indicating minutes, seconds, and hundredths of seconds. The integer seconds are advanced at every oscillation of the standard pendulum, and the minute at the end of each complete revolution of the seconds-wheel.

Presuming now we have this system of type-wheels in operation, it is necessary to print without disturbing their motion; especially is this true for the fast-moving type-wheel.

After a long series of experiments, during which the fast-moving wheel was detached and stopped in various ways, the impression was finally made from the spring of the hammer only, not allowing the blow to fall directly on the type, but arresting it about half an inch before it reached the top of the type. By this device, which is regarded of the greatest importance, the motion of the type is not disturbed an appreciable amount. Any number of impressions following each other in rapid succession does not disturb the fast-moving wheel the one-hundredth part of a second. By this plan none of the type-wheels are stopped or locked in the act of printing, and records of observations may follow each other as fast as the hammer can be made to deliver the blow.

The printing may be done directly by means of a strong electro-magnet, but the cost and trouble of keeping up a large battery for this purpose led us to do all the work mechanically, only using electricity as the governing power. Accordingly, a heavy running gear was built for raising the hammer, capable in its present form of delivering 2000 blows without winding. This gearing is entirely detached from the hammer when elevated, but is unlocked just before the hammer reaches the type, immediately raising it again. The time consumed for this operation is about three-tenths of a second, allowing therefore observations to follow each other at a minimum interval of one-half second. When the hammer is elevated it is locked by an electro-magnet, the operation of this magnet allowing it to fall and print. As the hammer is acted on by gravity alone, the armature time will be sensibly uniform.

The types are inked by means of small rollers, covered with cloth, resting against their rim, and revolving with the wheel by friction. These rollers require inking every two or three days.

The paper fillet, two inches in width, is wound on a spool holding sixty feet, and drawn between two rollers the same as a Morse Register. Our spool of paper will hold about 1200 observations including the spacing for different objects.

Every time the hammer falls the fillet is advanced one quarter of an inch, by the action of an escapement driven by a weight. The same escapement is also operated by an electro-magnet under the control of the observer, who by pressing a key is able to make spaces of any width between the prints.

To recapitulate, the following are the distinctive features of this mechanism :—

1st. Separate movements for the integer seconds and the hundredths of seconds; 2nd. The method of regulating the hundredths of seconds wheel by an electro-magnet in connection with the standard clock ; 3rd. The method of printing double or single numbers without stopping the type-wheels ; 4th. The method of striking the blow, indirectly using the spring of the hammer ; 5th. The method of elevating and locking the hammer.

The battery power required is about the same as for an ordinary chronograph. Three Grove elements or six Hill's elements

work the two electro-magnets well. A separate battery of about the same size is used for the hammer and fillet magnets.

In point of accuracy this machine leaves nothing to be desired, and is much beyond what we thought possible. From a vast number of experiments, made by recording automatically the beats of the standard clock, both at the middle and end of the oscillation, the mean error for a single print is found to be $0^s \cdot 013$, equal in this respect to the recording chronograph. The maximum difference in the records of the beats seldom exceeds $0^s \cdot 03$, and we believe this is as much due to the irregularity in the clock connexion, as in the running of the machine, since the same thing is found in ordinary chronographic records, where the measures are made from second to second.

The saving of time and labour by the use of a printing chronograph is very considerable. At the lowest estimate it does work equivalent to the labour of one person where three are employed at the same time. In our zone-work in former years, when the zone extended two hours in right ascension, it usually required the labour of two persons a whole day to convert the chronograph records into numbers and copy them on the blank forms. With the observations printed this labour is wholly dispensed with, since the "mean" is at once deduced from the printed records.

The machine is readily adjusted to indicate the same numbers as the clock's face, the type being so set as to print zero-hundredths when the pendulum is at its lowest point, where the magnetic circuit is completed.

In the observation of zone-stars or regular transit-work the type may be set to give the integer seconds of mean right ascension, so that the final reduction will always be a small quantity.

The constant use of this mechanism on every day and observing for six months, during which time more than twelve thousand records have been made, enables us to speak with confidence of its success, both as regards correctness in printing and in saving of labour.

Other things being equal it is found that for three observers twice as many observations can be reduced in the same time as when a recording chronograph is employed.

Formulæ for the Calculation of the Orbit of a Double Star.

By M. Annibal de Gasparis. (Translation.)

Having reduced by Herschel's method (*Memoirs R.A.S.* vol. v.) positions for six equidistant epochs, let $\rho_1, \rho_2 \ldots \rho_6$ be the distances and $\phi_1, \phi_2 \ldots \phi_6$ the angles of position. Denoting by ϖ_n the triangular area described on the plane of projection, and com-

prised between the distances ϱ_r and ϱ_s, we have $m_{rs} = \frac{1}{2} \varrho_r \varrho_s \times \sin(\phi_r - \phi_s)$. This being so, developing the area m_{rs} described on the plane of the orbit as a function of the radius vector and its derivatives, including the terms multiplied by the sixth powers of the times, we have to determine the ratios of the radius vectors r_2, r_3, r_4 the two equations,—

$$\frac{r_2^3}{r_3^3} = \frac{42\,m_{23} - 50\,m_{34} - 6\,m_{45} - 18\,m_{24} + 13\,m_{35} - 11\,m_{46} + 40\,m_{56} + 2\,m_{25}}{42\,m_{12} - 50\,m_{23} - 6\,m_{34} - 18\,m_{13} + 13\,m_{24} - 11\,m_{35} + 40\,m_{45} + 2\,m_{14}},$$

$$\frac{r_3^3}{r_4^3} = \frac{42\,m_{45} - 50\,m_{34} - 6\,m_{23} - 18\,m_{35} + 13\,m_{24} - 11\,m_{13} + 40\,m_{12} + 2\,m_{25}}{42\,m_{23} - 50\,m_{34} - 6\,m_{45} - 18\,m_{24} + 13\,m_{35} - 11\,m_{46} + 40\,m_{56} + 2\,m_{25}}.$$

These ratios being known, the direct calculation of the elements is quickly effected, since in fact the numerators and denominators are quantities of the third order. For this reason I thought it proper to use six positions (instead of four, which would have been sufficient) to attenuate in Herschel's curve the errors of observation, making use of a larger number of data. The memoir containing the necessary developments and other details will appear in vol. vi. of the *Atti* of the Academy of Sciences of Naples.

Occultation of Vesta, Dec. 30, 1871. By C. G. Talmage, Esq.

The occultation of *Vesta* was well seen here on the 30th Dec. The day had been very wet, but by 9·30 P.M. the sky was quite clear. *Vesta* was exceedingly bright right up to the Moon's limb.

The G.M.T. of immersion was

$$10^{h}\ 44^{m}\ 24^{s}\!\cdot\!8$$

and of emersion

$$11^{h}\ 52^{m}\ 52^{s}\!\cdot\!6.$$

Both exact. Power 80 on 10-inch refractor.

Note on the Variable Star S *Orionis.* By the Rev. T. W. Webb.

I have taken the earliest opportunity of reobserving the little variable star S *Orionis*, the character of whose light, suspected early in the year 1870, I was so fortunate to ascertain satisfactorily about this time twelve months ago. I find that it is now considerably smaller than on any previous occasion when I have looked for it; and as it is well situated among comparison-stars,

whose magnitude has been carefully determined by Mr. Baxendell, the investigation of its changes becomes an easy matter.

It may be remembered by those who have examined it, that it has a south-preceding companion, *d* of Baxendell, 11·1 mag.; and another north-preceding, *e*, 11·5 mag.; while at a greater distance we find *f*, nearly following, 11·6 mag.; *g*, north, 11·7 mag.; and two minuter points *h*, south-following, 12·5 mag.; and *k*, just beyond *e*, 12·6 mag. On the 5th instant I found that it was much fainter than its nearest neighbours *d* and *e*, and smaller than *f* and *g*, the lesser of which has 11·7 mag., but brighter than *h* and *k*, the larger of which is rated 12·5 mag.; but as it was nearer *g* than *h*, it may be estimated as a little brighter than 12 mag. Taken altogether, the observations point to a period of less than twelve months; but further examination is obviously required; and whether the star may even now have reached its *minimum* is uncertain.

Hardwick Vicarage, 12 *Dec*. 1871.

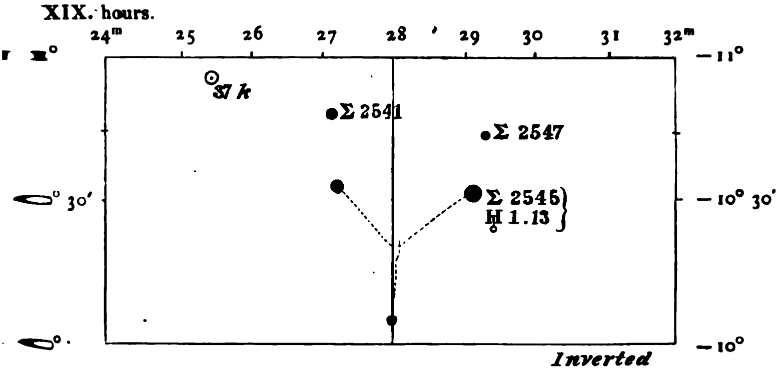
On the Identity of the Triple Star H. i. 13. By George Hunt, Esq.

In the *Monthly Notices*, November, 1862, the late lamented Mr. Dawes wrote a paper with the above title, on a triple star in *Aquila*. The perusal of that memoir leaves, I think it will be admitted, no doubt on the mind of the reader, that Mr. Dawes has clearly shown the identity of H. i. 13 with Σ 2545; yet it has always seemed to me, that he has not sufficiently dwelt upon *one* item in Sir W. Herschel's description of his star, which appears to establish the identity beyond all question. Sir W. Herschel says, "It is the last star of a telescopic trifolium *n* following *k*, similar to that in the hand of *Aquarius*."

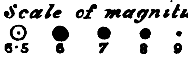
In the summer of 1868 I examined this region carefully with an Equatoreal of 4-inch aperture, by Simms, and soon made out Struve's three stars Nos. 2541, 2545, and 2547, but was at first much puzzled to identify Herschel's *trifolium*, until I put on a comet eye-piece magnifying twenty-seven times with a very wide field, when the beautiful trifolium was at once visible, having Struve's No. 2545 as "the last star." It occurred to me it would be interesting to map down to scale, all the stars closely north following 37 (*k*) *Aquilæ*, which could be found in Weisse's Bessel's Catalogue, and I venture to bring the result before this Society, as in some sort supplementary to Mr. Dawes' paper. In the accompanying small map, the stars are all inserted from Weisse's Catalogue, with the exception of Struve's 2545 and 2547, which are inserted from Struve's *Positiones Mediæ* (corrected for precession to 1825), and as my object is merely to show the *relative position* of the neighbouring stars, I have not applied the pre-

cession in R.A. and Dec. to the places of Weisse's Catalogue, of which the epoch is 1825. The *trifolium* is distinguished by the dotted lines. The stars in Weisse's Bessel included in the map, are, in the order of their R.A., *Hora* xix, No. 677 (= 37 *Aquilæ*), 692, 719 (= Σ 2541), 722, 744, 745, of which the last three and Σ 2545 or H. i. 13 constitute the *trifolium*. Struve's stars Nos. 2545, 2547, are not contained in Weisse's Bessel.

Mr. Dawes, in the paper above mentioned, after remarking that neither South, nor Struve, nor Herschel II, nor Smyth, saw the third star of this H. i. 13., proceeds to say that he saw it on August 19th, 1862, with a 4-inch aperture object-glass, by Cooke, " with so low a power as 135." This I can confirm with my own 4-inch Equatoreal.



Scale of magnitudes



In July, 1868, I find the following was inserted in my Note-book :—" Σ 2545. Best shown double with 120, third star glimpsed by averted vision, also with 200." In order to put it beyond doubt that the third star was actually seen, the same night I used Mr. Dawes' Solar eye-piece with a very small field and power 260, so as to exclude the close pair ; and under these circumstances the third star was (to use Admiral Smyth's phrase) "staring."

I cannot at all account for the R.A. and Dec. given in the Cycle under the numbers 702 and 703. While the description, " the three lie nearly in a line," shows conclusively that the worthy Admiral had his telescope directed to Σ 2547, the R.A. and Dec. given is exactly that of Struve's 2541 in the *Positiones Mediæ* for 1830, corrected for ten years' precession to bring it up to Smyth's Epoch for 1840, Σ 2547 having more than

2^m greater R.A. (This has a bearing on one of Sir John Herschel's queries, in the last *Monthly Notice* for Nov. 1871.) The R.A. of 703 does not seem to be that of any of Struve's three stars, the Dec. coinciding with that of Σ 2545 or H. I. 13. On the supposition that Smyth's 702 = Σ 2547, there is yet another difficulty not noticed by Mr. Dawes in his paper of Nov. 1862. Admiral Smyth gives the distance of the close pair = $3''\cdot 2$, while Mr. Dawes gives an approximative measure = $20'' \pm$, and Struve, as the mean of three sets of measures, = $20''\cdot 70$. Epoch 1830·02. Mr. Dawes, in his long and interesting note on this subject, in his Double-Star Catalogue, pp. 494-496, suggests a plausible explanation of these discrepancies. He says, "It is true that his (Smyth's) estimations of the distances of the stars composing Σ 2547 are very far from correct; but it seems highly probable that by some means his observations of Σ 2541 and Σ 2547 got mingled together—an accident the more likely, as they differ but little in declination. The existence of three close double stars within a small space, in all of which the smaller component has nearly the same relative position, has caused a degree of confusion which has no parallel in any other case."

In conclusion, I may as well mention two printer's errors. The mean place of 37 *Aquilæ*, in Mr. Dawes' paper, *Monthly Notices*, vol. xxii. p. 36, is wrong. The star is No. 1580 of the first Greenwich Seven-year Catalogue, and its mean place for 1860·00 is thus recorded: R.A. $19^h\ 27^m\ 24^s\cdot 28$ N.P.D. $100°\ 51'\ 46''\cdot 04$. In Mr. Dawes' Catalogue, star 2715 (= Σ 2545), the N.P.D. should manifestly be $100°\cdot 29'$, not as there printed $110°\ 29'$.

16 *Chad Road, Edgbaston, Birmingham,*
1872, *January* 3.

Note on the November Meteors. By Capt. Noble.

Watching almost persistently on the night of Monday, November 13, from $12^h\ 5^m$ to $13^h\ 15^m$, I failed to see a single meteor of any sort or description. The succeeding night was densely cloudy.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXII. *February* 9, 1872. No. 4

WILLIAM LASSELL, Esq., F.R.S., President, in the Chair.

Edward Henry Cooper, Esq., Markree Castle, Ireland;
J. Kennedy Esdaile, Esq., New University Club;
Isaac Engelson, Esq., Paris;
Capt. John Herschel, F.R.S., India;
D. Lindsay Lowson, Esq., 164 Kennington Road;
John Martin, Esq., 72 Adelaide Square, South Shields;
Capt. H. O'Reilly, Queensland;
E. Roberts, Esq., 3 Union Place, Blackheath Road; and
George Wall, Esq., Colombo, Ceylon,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Fifty-second Annual General Meeting of the Society.

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
December 31, 1870	192	305	10	3	510	44	554
Error in last Report	−2	...	...	...	...	...	...
Since elected ...	+6	+10	...	...	...	...	...
Deceased	−6	−8	−1	...	...	...	...
Removals	+2	−2	...	...	...	...	...
Resigned	...	−5	...	...	...	...	...
Expelled for non-payment of arrears, &c. ...	...	−6	...	...	...	...	...
Dec. 31, 1871 ...	192	304	9	3	508	44	552

Mr. Whitbread's Account as Treasurer of the I

RECEIPTS.

1871.		£ s. d.	£ s.
	Balance of last year's account		270 1
Jan. 6	By Dividend on £3400 Consols	50 3 0	
Apr. 6	By ditto on £5000 New 3 per Cents ...	73 15 0	
July 6	By ditto on £3400 Consols	49 14 6	
Oct. 6	By ditto on £5200 New 3 per Cents ...	76 1 0	
			249 13
	Received for arrears of contributions £210 11		
	192 annual contributions ... 401 2	611 13 0	
	Admission-fees	62 18 0	
	First years' contributions	39 16 0	
			714 7
	7 compositions		147 0
	Sale of publications:—		
	At the Rooms of the Society	5 11 6	
	By Messrs. Williams and Norgate ...	23 11 0	
			29 2
			£1410 4

Astronomical Society, from January 1 to December 31, 1871.

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Editor of Monthly Notices	60	0	0			
Assistant Secretary	130	0	0			
Commission on Collecting	36	0	0			
				226	0	0
Taxes:—						
Land and Assessed	7	0	9			
Income	1	17	8			
Poor Rate	11	17	0			
Other Parish Rates	5	9	8			
				26	5	1
Bills:—						
Strangeways, printer	220	6	3			
Rumfitt, bookbinder	17	12	7			
Malby, engraver	6	15	0			
Metcalf, ditto	12	5	0			
Insurance	7	15	6			
ne 9 Mr. Williams, in aid of printing Chinese Observations of Comets	50	0	0			
ly 20 Grant in aid of Eclipse Expedition, 1870	250	0	0			
				564	14	4
Miscellaneous items:—						
House expenses	25	1	10			
Stamps and postages	33	19	10			
Books and parcels	7	3	0			
Expenses of evening meetings	13	13	0			
Waiters attending meetings	3	17	0			
Coals and wood	12	0	0			
Gas	8	2	11			
Repairs	1	8	6			
Sundries	23	19	1			
				129	5	2
Mrs. Jackson Gwilt's annuity, 1 year ...				8	16	0
				955	0	7
Investment:— Purchase of £200 New 3 per Cents, 91¾				183	10	0
				1138	10	7
Balance at Banker's				271	13	7
				£1410	4	2

Examined and found correct, Jan. 25, 1872,

(Signed) H. Perigal, W. T. Lynn, W. B. Gibbs, } *Auditors.*

Assets and Present Property of the Society, January 1, 1872:—

	£ s. d.	£ s. d.
Balance at Banker's		271 13 7
3 Contributions of 7 years' standing ...	44 2 0	
5 " 6 "	63 0 0	
12 " 5 "	126 0 0	
2 " 4 "	33 12 0	
9 " 3 "	56 14 0	
9 " 2 "	37 16 0	
51 " 1 "	107 2 0	
Balances of several Accounts	19 2 0	
		487 8 0
Due for Publications		17 10 0
£5200 New 3 Per Cents (including Mrs. Jackson Gwilt's Gift, £300).		
£3400 Consols, including the Lee Fund (£100) and Turnor Fund (£500).		
Unsold Publications of the Society.		
Various astronomical instruments, books, prints, &c.		
Balance of Turnor Fund (included in Treasurer's Account)		167 12 9

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	11	XIII.	198	XXVII.	456
I. Part 2	51	XIV.	390	XXVIII.	416
II. Part 1	69	XV.	173	XXIX.	448
II. Part 2	33	XVI.	198	XXX.	297
III. Part 1	85	XVII.	181	XXXI.	173
III. Part 2	104	XVIII.	176	XXXII.	204
IV. Part 1	102	XIX.	183	XXXIII.	207
IV. Part 2	104	XX.	179	XXXIV.	193
V.	124	XXI. Part 1	216	XXXV.	167
VI.	150	XXI. Part 2	100	XXXVI. (with M. N.)	250
VII.	175	XXI. (together).	89	XXXVI. (without)	35
VIII.	161	XXII.	182		
IX.	164	XXIII.	176	XXXVII. Part 1	353
X.	174	XXIV.	182	XXXVII. Part 2	402
XI.	184	XXV.	196	XXXVIII.	438
XII.	189	XXVI.	200	XXXIX. Part 1	511

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle,
The *Baker* universal equatoreal,
The *Reade* transit.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatorial stand and clock movement.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatorial stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{3}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.
13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.
14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff, in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.
15. Level collimator, plain diaphragm.
16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.
17. Hassler's reflecting circle, by Troughton, with counterpoise stand.
18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.
19. 5-inch reflecting circle, by Lenoir.
20. Reflecting circle, by Jecker, of Paris.
21. Box sextant and 3-inch plane artificial horizon.
22. Prismatic compass.
23. Mountain barometer.
24. Prismatic compass.
25. 5-inch compass.
26. Dipping needle.
27. Intensity needle.
28. Ditto ditto.
29. Box of magnetic apparatus.
30. Hassler's reflecting circle, with artificial horizon roof.
31. Box sextant and 2¼-inch glass plane artificial horizon.
32. Plane speculum artificial horizon and stand.
33. 2½-inch circular level horizon, by Dollond.
34. Artificial horizon roof and trough.
35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.
36. A pentagraph.
37. A noddy.
38. A small Galilean telescope, with the object lens of rock-crystal.
39. Six levels, various.
40. 18-inch celestial globe.
41. Varley stand for telescope.
42. Thermometer.
43. Telescope, with the object-glass of rock crystal.

To these must be added the following instruments which had been employed in the Observations of the Total Solar Eclipse of 1870, and which, by a resolution of the joint Eclipse Committee of 1870, were transferred to the Royal Astronomical Society:—

Portable equatoreal stand,
Portable altazimuth tripod,
Four polarimeters,
Two Biquartz and Nicol's prisms,
Registering spectroscope, without prism,
Camera and chemicals, in box,
Two five-prism spectroscopes,
Cradle for telescope,
Eight-inch reflector and stand, with seven prisms,
Spectroscope,
A small box, containing—
  Three square-headed Nicol's prisms,
  Two Babinet's compensators,
  Two double-image prisms,
  Three Savarts,
  One positive eye-piece, with Nicol's prism,
  One dark wedge.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Fuller* theodolite, to the Director of the Sydney Observatory.
The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument,		No. 1, to Mr. Lassell.
Ditto	ditto	No. 2, to Mr. Huggins.
Ditto	ditto	No. 3, to Mr. Proctor.
Ditto	ditto	No. 4, to Rev. C. Lowndes.
Ditto	ditto	No. 5, to Mr. Birt.
Ditto	ditto	No. 6, to Rev. J. Cape.
Ditto	ditto	No. 8, to Rev. C. Pritchard.
Ditto	ditto	No. 9, to the Director of the Sydney Observatory.
Ditto	ditto	No. 41, to Rev. C. Pritchard.
Ditto	ditto	No. 43, to Mr. Huggins.

The 6-inch circular protractor, to Mr. Birt.

Lent on account of the Eclipse Expedition:—

Two polarimeters.		Col. Tennant.
One	ditto.	Mr. Ranyard.
One	ditto.	B. A. Eclipse Committee.
One Biquartz and Nicol's prism.		Col. Tennant.
One	ditto ditto.	Mr. Ranyard.
Registering spectroscope, no prism.		Col. Tennant.
Camera and chemicals, in box.		B. A. Eclipse Committee.
One five-prism spectroscope.		B. A. Eclipse Committee.
One	ditto.	Mr. Lockyer.

The Gold Medal.

The Council have awarded the Gold Medal to Signor Schiaparelli for his researches on the connexion between the orbits of comets and meteors. At the close of the ordinary business the President will explain to the Society the reasons which guided the Council in awarding the Medal to Signor Schiaparelli.

New Bye-law relating to the Award of the Medal.

At a Special General Meeting of the Society, held after the business of the Ordinary Meeting in June was concluded, the following Bye-law was submitted to the Fellows, and unanimously adopted.

In Section XVI. add:—

"75*. Notwithstanding the preceding Bye-laws, in cases where two or more persons have been jointly concerned in the production of any scientific treatise, or the carrying out of any research, work, or discovery, or have been the simultaneous but independent authors of any such treatise, work, research, or discovery, the Council may, under these circumstances, receive the nomination of such two or more persons as joint recipients of the Medal, and proceed thereupon in like manner as above mentioned with respect to the names of separate persons; and should the Medal be ultimately awarded to such joint authors, workers, or discoverers, an impression of the Medal shall be provided for and given to each of such joint recipients."

The Council have for some years past felt the importance of possessing the power to award a joint Medal when original researches have been performed by two or more persons, either when working conjointly, or when independent investigations on the same subject have been made. They are glad therefore to find that this feeling is also shared by the Fellows generally.

Printed Transactions of the Society.

Memoirs.

Part I. of Volume XXXIX. of the *Memoirs* has been published since the last Report. This Part contains a memoir by Professor Cayley "On the Graphical Construction of a Solar Eclipse," with an illustrative plate.

Many of the communications now printed in the *Monthly Notices* would, in former years, have been included in the *Memoirs*. This alteration in the manner of printing is generally both advantageous to the Author and the Fellows by the more rapid publication and distribution of matters of interest. A few of the subjects which have been before the Society, with others which bear upon the progress of Astronomy, are referred to in another section of the Report.

General Index to the Memoirs.

The Index to the *Monthly Notices* has been found so useful and acceptable to the Fellows, that the Council gave instructions to the Assistant-Secretary to prepare a General Index to the *Memoirs*, uniform with the Index to the *Monthly Notices*, including all the titles of Papers and the names of the authors contained in Volumes I.–XXXVIII. The work was completed by Mr. Williams during the recess, and placed in the hands of the Fellows in November last. The two Indexes thus form a complete catalogue of the numerous contributions towards the progress of our science which have been laid before the Society from its foundation to the present time.

Obituary.

The Council have to deplore the loss by death of a long list of Fellows, several of whom were distinguished astronomers and former officers of the Society. Sir John Herschel and Mr. Babbage were the last survivors of the small band of scientific men who met together at the Freemasons' Tavern on January 12th, 1820, "to take into consideration the propriety and expediency of establishing a Society for the encouragement and promotion of astronomy," a meeting which resulted in the foundation of the Royal Astronomical Society. The following list contains the names of the deceased Fellows:—

Fellows:—Charles Babbage, Esq., F.R.S.
Capt. J. Palladio Basevi, R.E.
Rev. A. W. Deey.
Augustus De Morgan, Esq.
Lt.-Gen. Sir W. T. Denison, K.C.B., F.R.S.
Earl of Dunraven, F.R.S.
Capt. R. W. H. Hardy, R.N.
Sir J. F. W. Herschel, Bart., F.R.S.
Sir A. Lang.
Sir Roderick I. Murchison, Bart, K.C.B., F.R.S.
Admiral Sir William Ramsay, K.C.B.
William Rutherford, Esq., LL.D.
Charles E. Smith, Esq.
Capt. David Smith.
Rev. William Taylor, F.R.S.
Rev. T. W. Weare.

Charles Babbage was born on St. Stephen's day, 26th December, 1792, and died on 18th October, 1871, aged nearly seventy-nine years. His parents were of good standing in the middle class. His early education, which commenced at five years, was conducted at various private schools, whence, after a

short sojourn with a private tutor, he was sent at the usual age to the University of Cambridge.

His love of investigation, which became the ruling passion of his life, was displayed when quite a child, and was first evinced by an experiment which he made in order to ascertain whether or not the Devil could really be raised in a personal form. The result, which was negative, removed a doubt which had obscured his religious belief, and his theological views seem from that time to have enjoyed undisturbed stability.

Babbage, like many men of great and original powers, made light of ancestry. Discussing the possibility that he might be descended from Tubal Cain, on the ground that he was, like himself, a great worker in iron, he pointed out that this hypothesis is shaken by the fact that to Tubal Cain is ascribed the invention of the *organ*, an instrument against which the subject of this notice, as is well known, carried on an incessant war.

It does not appear that Babbage owed either his tastes or his acquirements to external influences. Before going to Cambridge he had already, following his innate tendencies, plunged deeply into arithmetical and mathematical studies, reading such works as Humphrey Ditton's *Fluxions*, Agnesi's *Analytical Institutions*, Woodhouse's *Principles of Analytical Calculation*, Lagrange's *Théorie des Fonctions*, and the *Fluxions* of Maclaurin and of Simpson; and when he went to Cambridge the *dots* of Newton, the *d's* of Leibnitz, and the *dashes* of Lagrange were equally familiar to him.

Discursive and unassisted studies led, as they usually do, to independent habits of thought. His acquirements lay rather outside the established system of the University, and the difficulties he necessarily encountered seem to have been difficulties to the tutors to whom he vainly resorted for their solution. His confidence in the routine of the place was shaken, and he presumed to think for himself. Turning to the works of foreign mathematicians, he was "penetrated with the superior power of the notation of Leibnitz."

But he was not alone. A society for the promotion of analysis was formed at his suggestion, amongst the first members of which stand the names of Herschel, Peacock, D'Arblay, Ryan, Robinson, and Frederick Maule. They held meetings at which papers were read and discussed. The first volume of *Transactions* published was exclusively the work of Herschel and Babbage,—the latter proposing as its title "The Principles of D-ism in opposition to the Dot-age of the University." Persisting in these revolutionary schemes, Babbage, assisted by Peacock, translated the smaller work of Lacroix. Peacock, Herschel, and Babbage, afterwards published a collection of examples with their solutions: Peacock of the applications of the differential and integral calculus, Herschel of those of the calculus of finite differences, and Babbage of the solutions of functional equations. This work, by lightening the labours of the tutors, obtained their support, and

contributed much to the introduction of the system. "In a few years," says Babbage, "the change was completely established; and thus at last the English cultivators of mathematical science, untrammelled by a limited and imperfect system of signs, entered on equal terms into competition with their Continental rivals." Babbage had previously published an essay on the Calculus of Functions, and memoirs on the same subject. The creation of this as yet insufficiently-cultivated branch of analysis may in fact be considered as due to him.

Babbage's time and thoughts at Cambridge were not wholly engrossed by mathematics,—he worked with Herschel, under Smithson Tennant, at chemistry,—nor did his various studies suffice to employ his exuberant energy,—he joined in the amusements of the place, devoting himself particularly to boating with the vigour that so conspicuously characterized him.

It was here, in 1812 or 1813, that the first idea of a calculating machine germinated in his mind,—an idea that shaped his whole life, and fixed his name for ever in the history of Science.

It is impossible in a brief notice like the present to give a detailed account of this wonderful mechanical substitution for intellectual operations. A few leading facts may however be mentioned. The first engine designed by Babbage was named by him the "Difference Engine," its object being to compute tables by the method of differences, an object attainable by means little beyond that required for performing the arithmetical operation of addition. But the performance of this apparently simple task involves the process of carrying over the tens,—and to do this by mechanical means constituted the main difficulty of the undertaking,—a difficulty the solution of which occupied even the mind of Babbage during a long series of years.

After considerable progress had been made by Babbage in the construction of the "Difference Engine," an idea of a still higher order occurred to him,—that of an "Analytical Engine,"—a machine not for mere tabulating like the Difference Engine, but for calculating out any formulæ presented to it. Its principle seems to have been based upon, if not suggested by, that of the Jacquard loom, which, as Mr. Babbage points out, "is capable of weaving any design which the imagination of man may conceive." These designs, as is well known, are reproduced in the fabric by means of perforated cards, a distinct set of cards being required for each different design. Applying this principle to the Analytical Engine, each operation required two distinct sets of perforated cards, —one appertaining to the formula to be developed, the other containing the constants belonging to the particular case of which a solution was desired. The first set of cards prepared for a given formula would at any time recalculate that formula with whatever constants were furnished to it by the other set. The engine, therefore, was quite general in its application.

Besides performing these main operations, the machine was designed to execute the following subsidiary work:—To print on

paper one or two copies of its results; to produce a stereotype mould of the tables or results computed by it; and to punch on blank pasteboard cards or metal plates the numerical results of any of its computations. The speed with which its work should be executed was almost as astonishing as the work itself. It would multiply fifty figures by the same number in one minute.

Such was the complexity of these marvellous engines that even their gifted inventor found it difficult to interpret to himself his own drawings. He was also embarrassed by the number of modes of producing the same action suggested by his prolific inventiveness. Nor could he rest satisfied with less than a *demonstration* that the mode selected was the best. To meet these fundamental difficulties he found it absolutely necessary first to invent a system of Mechanical Notation, such that when applied to his drawings it should give at once, by simple inspection, without any verbal description whatever, all the elements required for explaining the nature, position, object, and effect of every part of the machine depicted.

His paper, descriptive of this system, which is of universal application, "On a method of expressing by Signs the Action of Machinery," was read before the Royal Society, on the 16th of March, 1826. This memoir alone would confer celebrity on its author.

Mr. Babbage was justly proud of his system of Mechanical Notation, and was fond of performing the following feat to exhibit its powers. He would request his visitor to select one from the hundreds of drawings before him, executed perhaps ten years previously. Turning his back on the drawings, he desired his visitor to place his finger on any part of it. He then asked him a few questions as to the letters and symbols on or near that part,—whether they were upright or sloping, large or small, Arabic or Roman, and so on,—and he then, without any apparent exercise of thought and without any hesitation, described the part, its function, position, and relation to the whole. It was his opinion that this system of Notation was as indispensable to the designing of the engine as tools were to its construction.

The history of these marvels of intellect has often been told, and is sad indeed. For a full and trustworthy account of it, the reader is referred to Weld's *History of the Royal Society*. A mere outline only can be here given.

It was proposed that the Government should contribute to the construction of the "Difference Engine." The question was referred to a committee of the Royal Society, consisting of Sir H. Davy, Mr. Brande, Mr. Combe, Mr. Francis Baily, Mr. Brunel, Major Colby, Mr. Davies Gilbert, Sir John Herschel, Captain Kater, Mr. Pond (Astronomer Royal), Dr. Wollaston, and Dr. Young. This most competent tribunal reported favourably on the project, stating that "they considered Mr. Babbage as highly deserving of public encouragement in the prosecution of his arduous undertaking." Funds were accordingly provided by Govern-

ment from time to time, but being never adequate in amount, Mr. Babbage himself contributed largely to the expenses from his own resources. Time went on, the works proceeded, and the design not only grew, but became productive of new ideas. The conception of the Analytical Engine resulted. No sooner did this advance in the subject appear to its author capable of practical execution than, with characteristic candour, he felt bound to communicate it to the Government, as affecting the question whether, with such a prospect of almost boundless powers of mechanical calculation before them, they would persevere in the construction of the more limited Difference Engine. For seven years Mr. Babbage's applications to various ministers and administrations failed to elicit an answer to this simple question. At length, on 4th Nov. 1842, the answer came from Mr. Goulburn (Chancellor of the Exchequer in Sir Robert Peel's Cabinet), regretting that, on the grounds of expense, the completion of the machine must be abandoned, and offering to place at Mr. Babbage's disposal all that existed of it. This latter offer Mr. Babbage declined, feeling that he had no right to accept public property which he perhaps considered the Government had no right to bestow upon him.

It is no part of our intention here to revive the personal animosity which the whole management of these transactions naturally excited. But we may extract a lesson from them. It must be evident to any one who studies the documents connected with the case that the inconsistencies and narrowness of view apparent in them arose mainly from the fact that there was no provision in the Government for the proper consideration of such a question—no minister, no department, no official advisers, who could properly be made responsible for thoroughly investigating the matter, and for pronouncing a well-weighed decision upon it. The consequence was, that in the press of party politics, no one minister would give the requisite attention to such a matter. The advice of the Royal Society, twice given in most decided terms in favour of Mr. Babbage's proposal, not being official, was ultimately disregarded; and the pretext of expense, always ready when the object of the expenditure is not understood, was, as has often happened in similar cases before and since, turned to account.

The deficiency in our administration, which operated unfavourably in this famous case, still exists. In a country which produces, it is true, but one Babbage in an age, yet teems beyond all other countries with inventors ready to devote their genius to the public good, there exists, as yet, no public functionary charged with the examination of the thousands of inventions, and discoveries, and projects of which, with enormous advantage to the country at large, the Government might readily avail themselves. The archives of this Society are not without records of difficulties in connexion with its own branch of science, in which the want of

a minister competent to deal with astronomical undertakings requiring State aid, has been felt.

To return to the subject of this notice. Mr. Babbage states that the following sums were spent upon the Difference Engine: —by the Government, about 17,000*l.*; by himself, upwards of 20,000*l.* The part completed was deposited in the Museum of King's College. Thence it was removed, through the great personal exertions of the late Mr. Gravatt, C.E., to the International Exhibition of 1862; after the closing of which it was transferred to the South Kensington Museum, where it now remains.

Babbage now turned his mind to the Analytical Engine; but experience told him how great must be the cost of such a work, the difficulties of which were moreover increased by the dearth of skilful draftsmen drawn away by the railroad mania, then at its height. In this doubt he had recourse to the advice of his mother. It was given in words worthy the mother of such a man: "My dear son," she said, "you have advanced far in the accomplishment of a great object, which is worthy of your ambition. You are capable of completing it. My advice is, Pursue it, even if it should oblige you to live on bread and cheese." He nobly pursued it accordingly, with unremitting toil of mind and body, as long as mental and bodily powers remained to him.*

Labours such as these would have sufficed to occupy the energy of most men. Not so Babbage. His other contributions to science and to human knowledge generally were so extensive that only some of the principal results can here be mentioned.

His memory will always be revered in this Society, in consequence of his being one of its founders, and one of its most steady supporters, until the latest period of his active life. He was very proud of being the recipient of the first medal ever awarded by the Society.

In 1828 Mr Babbage was appointed to the Lucasian Professorship of Mathematics in the University of Cambridge, a chair which had been previously filled by some of the most distinguished mathematicians of this country, including Dr. Barrow, Sir Isaac Newton, Prof. Woodhouse, and the present Astronomer Royal. This chair he held till 1839.

He took a prominent part in the founding of the British Association for the Advancement of Science, the Statistical Section of which he established. He was also the original proposer and founder of the present Statistical Society of London.

He found time to write much. His *Economy of Manufactures* is a standard work of the highest authority on the subject. His *Passages from the Life of a Philosopher*, written more

* The best account of the principles on which this engine is based is that by Menabrea, of which a translation into English, and at Babbage's suggestion extensively annotated, was made by Lady Lovelace, the daughter of the poet Byron.

recently, though containing some personal allusions which are to be regretted, but hardly to be wondered at, is one of the most interesting and amusing autobiographies ever written. His contributions to scientific and learned societies were very numerous, exceeding perhaps one hundred. They relate to almost every branch of science, physical and social; and their author being one who disdained smatterings of knowledge, and searched always for the elementary basis of every subject he undertook to examine, there is not one of these papers that does not exhibit the mastery of an exact and comprehensive mind.

The indirect effects of Babbage's principal labours cannot be estimated, and will probably never be duly appreciated but by a few. The discoveries and improvements introduced by him in the art of metallic construction are innumerable. The shaping of metals by automatic machinery, now so well understood, was an art in its infancy when Babbage was first compelled, by the necessities of his designs, to turn his attention to it. Others laboured contemporaneously at the same problems, and the names of Clement, Bryan Donkin, Maudsley and Whitworth, cannot suffer by being coupled with that of Babbage.

It is impossible in a small compass to give an adequate estimate of such an intellect. Its salient features were universality, grasp, method, patience. What do not these four qualities, inspired by intense love of truth and never-flagging enthusiasm, and sustained by enormous power of work, imply? A man to whom no branch of knowledge was distasteful or difficult,—none too large or too minute,—who by the searching faculty of analysis could, sweeping aside superficial subsidiary complexities, dive straight to the fundamental principle,—and to whom weariness and wavering were unknown weaknesses,—possessed endowments before which most obstacles must have yielded. All careers were open to such a man. In any he must have succeeded. Unhappily for himself, he chose a path which, though no other man that ever lived could perhaps have trodden it as he did, led only to loss of fortune and embitterment of mind; and though to fame, to the fame which to many will be for ever a mystery, and to the few who can prize it perhaps a regret. It is as a scientific mechanician that Babbage will hereafter, no doubt, be regarded. In this field he had no equal. The combination of the highest mathematical genius with a lofty and vivid imagination, and the power of descending to the most minute details of technical construction, has never been observed in the same degree, and well-balanced harmony of proportion, as in Babbage.

Some idea of Babbage's versatility may be gathered from the mention of some of the subjects on which he wrote—Glaciers; Uniform Postage; Parcels Post; Submarine Navigation; Magnetic and Electric Rotations; Light-houses; Light-signals; Telegraphs; Geology; Miracles; Monopolies; Locks and Lock-picking; Division of Labour; Taxation; Commerce; Wood-engraving; the Diving Bell; Games of Skill; besides numerous

contributions to Astronomy, Mathematics, and Mechanics. He wrote also the ninth *Bridgewater Treatise*. It may safely be said that he never wrote without previous study, and that nothing he has written can be read without both stimulating thought and conveying instruction.

As a man he was equally admirable—generous, just, truth-loving, disinterested beyond most men—he had the faculty of endearing himself to those who knew him. Though immersed in the most abstruse pursuits, he was yet full of human sympathies, mixing habitually during his most busy years, in the society and even in the gaieties of the Metropolis. His conversation was so easy and his nature so genial, that, wherever he went, he was a centre of attraction, particularly with the fair sex, with whom he was always a favourite. In the various notices of him that have appeared, one of his most marked qualities is strangely overlooked—namely, his love of humour; this was so strong a characteristic that, to omit it, is to omit half the man. In his most serious, and even in his most indignant moods, he would always turn aside to exhibit the ludicrous side of the question; and, to a deep-seeing intellect like his, what question has not its laughable aspect? In conversation, as in his writings, he was very fastidious in the choice of his language, which possessed the charm of power united with simplicity in a high degree. Though he entertained a high estimate of his own powers and acquirements, he was always as ready to learn as to teach; and he would seek instruction from the humblest sources. An artisan skilful in some special matter had in him a patient and deeply interested listener.

This notice, brief as it is, would hardly be complete without some allusion to Mr. Babbage's constant endeavours to suppress street music and street nuisances—for, to a considerable mass of his countrymen, he is only known as the prosecutor, or, as they may have thought him, the persecutor, of organ-grinders. When it is stated that, according to the estimate of a man so remarkable for exactness and truth, one quarter of his time was wasted by the interruptions caused by such noises; and when it is considered what that implies in the case of one who never worked languidly, but whose every available moment was employed in deep and sustained thought, the extent of this grievance will be properly appreciated. The mental distraction which external noises caused him was part of a highly sensitive organisation,—an organisation frequently associated with yearning aspirations after unattainable refinement and perfection. His case is that of many students and workers. John Leech, the famous artist, suffered severely, if not fatally, from the very same cause. Mr. Babbage put the law in action, and it utterly failed to protect him. It is not surprising that he should complain bitterly—as less eminent men than he do daily when they find that in London, beyond any place in the civilised globe, the comfort of the many is deliberately sacrificed, in this, amongst many other ways, to the vested interests of a few.

Those who only know Babbage's name in connexion with this matter, and imagine him to have been a mere peevish visionary, require to be told that no man ever more truly loved his kind, and that few men have devoted transcendent powers like his with such disinterestedness, such tenacity, and such noble self-sacrifice, to what he believed to be the profit and the elevation of humanity.

A. S.

Captain James Palladio Basevi, R.E., was a son of the celebrated architect who designed the Fitz-William Museum at Cambridge, and other important buildings, and lost his life by falling from the tower of Ely Cathedral while superintending the restoration of that edifice.

James Basevi was distinguished as a lad for more than ordinary talent, and for his mathematical abilities. After passing with great credit through Rugby, the Cheltenham College, and the Honourable East India Company's Military Seminary at Addiscombe, he obtained a commission in the Corps of Engineers as first of his term, and went out to India in the year 1853. Three years afterwards he was appointed to the Great Trigonometrical Survey of India, upon which he continued to serve up to the time of his death. He was particularly qualified for this branch of the public service, and his excellent abilities being associated with remarkable powers of perseverance, and an entire devotion of self to duty, he was soon acknowledged to be one of the most valuable officers of the Department with which his interests were inseparably bound up. He took a prominent part in each of the various branches of the operations, the triangulations, linear measurements, topography, and mathematical reductions; he completed two chains of principal triangulation of an aggregate length of nearly 300 miles, mostly over very difficult ground; he supervised the measurement of a base-line at Cape Comorin, with Colby's apparatus of compensation-bars and microscopes, and wrote a valuable paper on the probable errors of the measurement, which is printed among the Appendices to *The Account of the Operations of the Great Trigonometrical Survey of India*, Vol. i. Dehra Doon, 1870.

In 1864 Captain Basevi was selected to undertake the Pendulum Operations which were commenced at the suggestion of the President and Council of the Royal Society, for the purpose of determining the variations of the force of gravity at certain stations of the great meridional arc in India, which was measured by Colonels Lambton and Everest, and at stations on the coasts and in the interior of the British territories in Asia. The object of these operations was two-fold; first, to obtain additional data to combine with the result of similar operations in other countries for determining the figure of the Earth; and, secondly, to ascertain the magnitude of the variations which are superposed, in certain localities, on the normal increase of gravity from the equator to the poles, in order that, by comparing the force of gravity on

high-table lands, on plains little elevated above the sea-level, and on oceanic islands, some light might be thrown on the structure of the Earth's crust.

Captain Basevi entered on this work with great ardour and devotion, sparing no pains to secure results of the utmost accuracy and precision humanly attainable. His observations of pendulum and clock coincidences were continued over at least twelve days at each station, and for ten hours daily he was never absent for more than a few minutes at a time from his pendulums, the observations recurring at every hour and a half; in the evenings two hours more were devoted to star-observations for time; and the programme of work was never deviated from with the object of curtailing it, though not unfrequently additional observations were taken. Besides this, very elaborate and laborious investigations were made for the purpose of determining the values of the coefficients of temperature and pressure, and other constants, a knowledge of which was required for the reduction of the observations.

By the spring of 1869, Captain Basevi had completed the operations on the Indian Arc, and found at the northern extremity thereof a deficiency of density as the stations approached the Himalayan Mountains, and conversely an increase of density at the southern extremity as the stations approached the ocean. These results point to a law of diminution of density under the elevated, and of increase under the depressed, parts of the Earth's surface; they were subsequently corroborated by observations on the Island of Minicoy, and at various points on the coasts of the Peninsula. Comparing these *inter se*, and with the results at the inland stations, gravity was found to be greater on the island than on the corresponding coast station, and always greater on the coasts than inland on the same parallel of latitude.

Thus far, however, no very high altitudes had been visited; arrangements were therefore made to secure the crucial test of observations at altitudes exceeding 15,000 feet on the great table-lands in the interior of the Himalayan Mountains. This would complete the work in India, after which the pendulums were to be swung at Aden and in Egypt, and finally at the Greenwich and the Kew observatories, that the operations might be combined with those of Kater, Sabine, Biot, Luetke, and Sawitsch.

In the spring of the present year Captain Basevi proceeded to the interior of the Himalayan Mountains, *viâ* Kashmir and Ladak. He completed an excellent series of observations at a point in N. lat. 33° 16′, and long. 77° 54′, on a plateau at a mean elevation of 15,500 feet above the sea-level, where the force of gravity (reduced to sea-level) was found to be only as much as the normal force in the parallel of latitude of 27°, or 6° to the south of that of the station of observation. He then proceeded through the Changchenmo Valley to a plateau nearly 17,000 feet high on the borders of the Chinese territories,

and was commencing to take observations when he died suddenly after a very short illness. His death is clearly to be attributed to the privations which he had to endure in a highly rarefied atmosphere under exposure to great severities of climate, in a barren and desolate region almost wholly devoid of every necessary of life. It is the more to be regretted in that it occurred when his work was all but completed, and he was eagerly looking forward to a return to his native land with the gratification of having satisfactorily accomplished an arduous and difficult series of operations.

He died in the prime of life, at the early age of thirty-nine years. J. T. W.

EDWARD WILLIAM BRAYLEY* was the son of the late Edward Wedlake Brayley, well known as the author and editor of numerous topographical and antiquarian works, and for many years Librarian of the Russell Institution.

The subject of the present notice was born in London in 1801. He at the first took great interest in the studies so ardently pursued by his father, but subsequently devoted himself entirely to science, and became a student under Professor Brande, at the Royal Institution, where he made the acquaintance of Faraday and other eminent men. He also attended the lectures of Professor Millington, and attracted the attention of that distinguished philosopher, who was remarkable for the assistance he rendered his inquiring pupils. In 1824, when only twenty-three years of age, he became a Member of the Meteorological Society, and engaged in its management with Dr. Birkbeck, Mr. Luke Howard, and other distinguished cultivators of Meteorological Science. He shortly after became a lecturer on various subjects connected with science, and was closely engaged as such by the various scientific and mechanics' institutes established about that time by Dr. Birkbeck in the Metropolis and the chief towns of the kingdom. In the year 1834 the managers of the London Institution determined to engage two principal librarians, the one to take charge of the department of general literature, the other of science. Mr. R. Thomson was appointed to the former office and Mr. Brayley to the latter. He was also engaged to lecture at the Institution, and generally delivered a course of lectures on physical science during the season. He became a Fellow of various scientific and literary bodies, and joined this Society in November 1866.

On the death of Mr. Thomson, in 1865, Mr. Brayley was appointed sole principal librarian, and also Professor of Physical Geography and Meteorology to the Institution. For more than a year before his death he became subject to attacks of occasional faintness, and on the 1st of February, 1870, he expired, after twenty-four hours of severe suffering, from angina pectoris.

* This obituary notice of Mr. Brayley was received after the publication of the last Annual Report, in which it ought to have appeared.

The Rev. Alfred W. Deey, M.A., was born at Norwood on the 29th of January, 1838. He was educated at the Merchant Taylors' School, in the City of London, and in May, 1856, he matriculated at the University of Oxford, gaining a Postmastership at Merton College.

At Merton he applied himself with diligence to his College studies, and he won the esteem and respect of his contemporaries by his genial, kindly qualities, and great independence of character. In June, 1860, he took his degree, his name appearing in the Second Class Mathematical Honours. On Trinity Sunday, 1861, he was ordained by the Bishop of Bath and Wells to the Curacy of Chaffcombe, near Crewkerne, Somerset, and at the same time he accepted the post of Mathematical Master in the Crewkerne Grammar School.

The following year he entered Priests' Orders, and he remained in that position for nearly three years, during which time his very agreeable mode of imparting knowledge and his sterling good qualities were thoroughly appreciated by the able and respected Head Master, the Rev. Dr. Penney, and were further acknowledged by the boys themselves on his leaving them, by their presenting him with a handsome telescope.

In April, 1863, he proceeded M.A., and shortly afterwards was appointed Senior Curate of Alton, Hants. Here he laboured for several years, and devoted himself, with all the ardour of his nature, to the faithful discharge of the various duties that devolved upon him in such a large parish. In July, 1869, he was presented to the vacant living of Hartley-Mauditt, near Alton, and in 1870 he was also appointed Vicar of the adjoining small parish of West Wooldham.

From quite early years he had manifested a strong bent in favour of astronomical studies, and he never lost an opportunity of improving himself in them. In January 1866 he was elected one of the Fellows of this Society, and his name may often be seen in the *Astronomical Register* as a correspondent, particularly in a lengthened reply, in the April number of 1870, to a letter from Mr. Elvins on the motion and physical constitution of the Moon. With the assistance of a powerful telescope he made a frequent examination of the spots on the Sun, and made sketches of their appearance from time to time, until interrupted by illness. Mr. Deey died at the early age of thirty-three, respected by his parishioners and numerous friends, who deplore his premature loss.

Professor De Morgan was born at Madura, in Southern India, in June, 1806. Several members of his father's family had distinguished themselves in the Indian army. His great-grandfather and grandfather had fought under Warren Hastings, and his father, Colonel John De Morgan, was a man of considerable energy. But it was from the maternal side that he must have inherited his mathematical powers. His mother was a grand-

daughter of James Dodson, F.R.S., author of the *Anti-logarithmic Canon*, a pupil and friend of De Moivre's, and master of the Mathematical School of Christ's Hospital; he was also the chief founder of the Equitable Life Assurance Company, for which he calculated the first tables.*

In August, 1806, Colonel De Morgan and his wife returned to England, bringing with them a daughter, and their infant son, Augustus, then three months old. He had been born with but one eye, but was a strong child. On arriving in England, they first settled at Worcester, and then lived rather a migratory life, moving from place to place in the west of England. When the Professor was between four and five, he remembered his father giving him his first lessons in arithmetic. After a few years Colonel De Morgan returned to India, again leaving his wife and children in this country. And while on his way to England in 1816, he was taken ill and died at sea off St. Helena, leaving his widow with four children. The little Augustus was then a boy of ten, and passed from one private school to another, being crammed with general knowledge, Latin, Greek, and even Hebrew, before he was fourteen. In after life he had a strong aversion to the overstrained education of young children; he hated cramming, and the competitive examinations of the present day, believing them to be an almost unmixed evil, against which, however, it was in vain to remonstrate; the errors which he deplored would only become apparent by their fatal results in the next generation. At the age of sixteen and a half he passed on to Cambridge, where he entered at Trinity College. His rooms were in the south-east corner of the Great Court, then called "Mutton Hole Corner," which he used to affirm was a corruption of Merton Hall Corner. Here he spent his undergraduateship, and greatly developed his love for mathematics, which had already begun to show itself before he left school. He, however, indulged his musical tastes, learning to play the flute, of which he is said to have been one of the best amateur players in England, and also spending much of his time in general reading, which greatly interfered with his study for honours. One of his college lecturers was Mr. Airy, who had been senior wrangler of the year in which the young De Morgan entered.

He had not completed his twenty-first year when he gained the fourth place in the mathematical tripos of 1827, the order of the list being Gordon, Turner, Cleasby, De Morgan. The place of the youthful wrangler, though it failed to declare his real

* He wrote many books besides the *Anti-logarithmic Canon*, which was a work unique of its kind, containing the number to eleven figures corresponding to every logarithm from ·00001 to 1·00000. The author is said to have corrected the faults in most copies with his own hand. He wrote a quarto book on the method of book-keeping, of which Professor De Morgan says in his *Arithmetical Books from the invention of printing to the present time*, 1847, at page 71, "As far as I can find, this is the first book in which double entry is applied to retail trade."

power or the exceptional aptitude of his mind for mathematical study, would, however, have been sufficient to have secured for him a fellowship, and he, no doubt, would have found a congenial field of labour within the walls of his university, if his conscientious scruples had not prevented his signing the tests which at that time were required from those who took up their degree of M.A. as well as from all Fellows of Colleges.

In his later years he termed himself with characteristic pleasantry, "A Christian Unattached," and the considerations which caused him to describe thus negatively his religious position were so far operative on his conscience in early manhood, that he forbore to secure a high wrangler's ordinary share of University preferment by formal, and what would have been, to him, insincere subscription.

In 1863 he wrote, "What is belief? A *state* of the mind. What is it often taken to be? An *act* of the mind. The imperative future tense—I will believe— thou shalt believe, &c.— which has no existence except in the grammar-book, represents a futile attempt which people make upon themselves and upon others." This sentiment, expressed so forcibly in his later life, was the moving power which, when he was on the threshold of manhood, caused him to withdraw from the University in which he would, doubtless, have become a powerful leader of mathematical thought, had his religious feelings been less acute or his principles more elastic.

On coming to London, he entered at Lincoln's Inn, and would have been compelled to forsake mathematics for the study of the law, but that, in 1828, the University of London, now University College, was founded, and he was appointed to the mathematical chair. In May of the same year he became a Fellow of this Society, and in the following November gave his first introductory lecture at University College. In his own words, he then "began to teach himself to better purpose than he had been taught, as does every man who is not a fool, when he begins to teach others, let his former teachers have been what they may."

In 1837, ten years after his appointment at University College, he married Sophia Elizabeth, the daughter of Mr. William Frend,* formerly a Fellow of Jesus College, Cambridge, author of several mathematical books, and Actuary of the Rock Life Assurance Office. The professional experiences of his father-in-law opened to De Morgan a fresh sphere of labour, in which he turned his mathematical acquirements to account in the service of many of the London Insurance Companies. In this new field of labour he might, doubtless, have made a much larger income than he could ever hope to derive from his professorship at University College, but he was devotedly attached to the principles of the new institution, and made it his first duty to advance its interests.

* Mr. Frend was a fellow of this Society; his obituary notice, written by Professor De Morgan, will be found in vol. v. of the *Monthly Notices*, p. 144.

He was so punctual and so regular in the performance of his college duties that his passage to and from his classes served as a time-piece to observant students. Soon after eight o'clock every morning he might have been seen passing along the railed enclosure of University College, so absorbed in thought that his nearest friends might pass him without being recognised.

He remained a firm supporter of the institution and its principle of no religious tests till the year 1866, when the Council, in making an appointment to the chair of Logic and Mental Philosophy, refused, as the Professor believed, one of the candidates on account of his religious opinions. Professor De Morgan, acting under the conviction that the Institution to which he had devoted the best years of his life, was forsaking the leading principle on which it had been founded—a principle which from early manhood had influenced him, and was now dearer to him than ever—felt it his duty to resign his professorship.

And thus, though deeply disappointed and grieved at the step he felt forced to take, Professor De Morgan, who had for nearly forty years been the chief honour and ornament of University College, left it, and, we are informed, never afterwards entered its gates.

To estimate the energy of the Professor, we must look at him not only as a teacher of mathematics, but as a mathematician, an actuary, a logician, an historian, a biographer, and a bibliophile.

First, then, as a teacher of mathematics; perhaps no man has been more successful in training distinguished mathematicians, amongst whom we may mention the names of Professor Clifton, Judge Hargreave, Mr. Routh, and Mr. Todhunter. Professor Sylvester also attended his lectures, though the relationship of professor and pupil did not in this case last very long. He had a method of interesting his hearers in the subjects on which he lectured, and of making them love mathematics for its own sake, to which few other men have ever attained. He devoted more time and labour to the logical processes by which the various rules are demonstrated than to the more technical parts of his subject, though of these too, in their proper place, Professor De Morgan was never unmindful, spending the greatest care on teaching the art of rapid and accurate computation.

His exposition of the elementary principles of the Differential Calculus, and of the logical processes of his Double Algebra, was most masterly and exhaustive, and was often enlivened by such humorous illustration that it never failed to impress itself upon the minds of his hearers. The subject-matter of every lecture which he delivered was entered by him in a note-book, and was sent into the library of the college for the benefit of his pupils while writing out and expanding their own notes.

As a mathematician his work was so various that it would be difficult for any one man to review it; but we may allude, in passing, to his double algebra, which was certainly the forerunner

of Quaternions, and contained the complete geometrical interpretation of the $\sqrt{-1}$. Sir William Rowan Hamilton, in the Preface to his *Lectures on Quaternions*, at p. 41, says: "But I wish to mention that, among the circumstances which assisted to prevent me from losing sight of the general subjects, and from wholly abandoning the attempt to turn to some useful account those early speculations of mine on triplets and on sets, was probably the publication of Prof. De Morgan's first paper on the Foundation of Algebra, of which he sent me a copy in 1841." And at p. 64 of the same Preface he says, in speaking of this theory of Sets of which he considered Quaternions (in their *symbolical* aspect) to be merely a *particular* case, "Before the publication of those *sets*, the closely connected conception of an '*algebra of the* nth *character*' had occurred to Prof. De Morgan in 1844, avowedly as a suggestion from the Quaternions.' A great portion of his original investigations will be found in the form of communications to the *Transactions of the Cambridge Philosophical Society*."*

As a writer of mathematical text-books he took the highest rank, his books being more suitable, however, for teachers than for pupils. They were characterised by extreme clearness, exhaustiveness, and suggestiveness. Perhaps those best known are his *Elements of Arithmetic*, published 1830; his *Elements of Algebra*, published 1835; and his *Differential and Integral Calculus, with Elementary Illustrations*, which is a perfect mine of original thought, and in which some of the most important extensions which the subject has since received are distinctly foreshadowed. It was published by the Society for the Diffusion of Useful Knowledge.

His first mathematical text-book was a *Translation of the Elements of Algebra by Bourdon*, published when he was only twenty-two years of age. He wrote text-books upon trigonometry, double algebra, the theory of probabilities, connexion of number and magnitude, projection, and the use of the globes; besides which he edited more than one table of logarithms, and wrote prefaces and notes to the books of other mathematical authors. The list of his works and their various editions occupies nine pages of the British Museum Catalogue.

As an actuary he occupied the first place, though he was not directly associated with any particular office; but his opinion was sought for by professional actuaries on all sides, on the more difficult questions connected with the theory of probabilities, as applied to life-contingencies. In 1830 he wrote for the *Cabinet Cyclopædia* his "Essay on Probabilities,"—a book which still retains a high place among the literature of insurance offices.

* The Royal Society's Catalogue of Scientific Papers gives a list of forty-two communications by Prof. De Morgan, mostly in the following publications:—*The Transactions of the Cambridge Philosophical Society, The Cambridge Mathematical Journal, The Cambridge and Dublin Mathematical Journal, The Philosophical Transactions*, and the *Memoirs* and *Monthly Notices of the Royal Astronomical Society*.

As a logician he was well known, and his *Formal Logic*, together with the treatise of Dr. Boole, may be said to have created a new era in logical science. His controversy with Sir William Hamilton will long be remembered.

As an historian and biographer the *English Encyclopædia* says of him, that "he had a great affection for, and an extensive and minute erudition in, all kinds of literary history, biography, and antiquities." He was one of the most extensive contributors to the *Penny Cyclopædia*, many of the articles of scientific biography having been written by him, as well as most of the mathematical and astronomical articles. His contributions to the work are said to amount to one-sixth of the whole Cyclopædia. The lives of Newton and Halley, in Knight's *British Worthies*, were also from his pen. Besides these he published an index of the correspondence of the scientific men of the seventeenth century; and a book entitled *General Information on Subjects of Chronology, Geography, Statistics, &c., References for the History of the Mathematical Sciences*, 1842.

As a bibliophile, his *Arithmetical Books from the Invention of Printing to the present time*, 1847, and his *Budget of Paradoxes*, will long remain celebrated. A reprint of the latter, greatly enlarged from manuscript notes left by the Professor, is now being taken through the press by his widow. He was the possessor of a very choice collection of mathematical works, which have, since his death, been purchased by Lord Overstone, and presented to the University of London, where they now form a De Morgan Library ; most of the volumes contain bibliographical notes and sometimes quaint and humorous scraps from newspapers or other books pasted into their covers.

He was a true bibliophile, and revelled in all the mysteries of watermarks, title-pages, colophons, catchwords, and the like. He wrote in the preface-dedicatory of his list of arithmetical books, "The most worthless book of a bygone day, is a record worthy of preservation. Like a telescopic star, its obscurity may render it unavailable for most purposes ; but it serves, in hands which know how to use it, to determine the places of more important bodies."

In addition to this, the Professor contributed largely to the *Philosophical Magazine*, the *North British Review*, the *Athenæum*, the *Companion to the Almanac*, and *Notes and Queries*. The *Athenæum** says of him, that if all the articles which he contributed to literary and scientific journals were collected together, there would be found such a mass of literary achievements as seldom comes from the pen of a man whose sole business is to write for journals. He also compiled a *Book of Almanacs*, with an index of reference, by which the almanac may be found for every year up to A.D. 2000, with means of finding the day of the new moon from B.C. 2000 to A.D. 2000, published 1851.

* March 25, 1871, p. 370.

The Professor also devoted much of his time to the business of this Society. For a period of more than thirty years he was officially connected with it, sometimes serving upon the Council, sometimes as Vice-President, and for many years as Secretary, in conjunction with Mr. Bishop and Admiral Manners, and during this period many of the volumes of the *Monthly Notices* were produced under his editorship.

He was the first President, and took great interest in the formation of the London Mathematical Society, of which his son, the late George Campbell De Morgan, was one of the first honorary secretaries. Soon after the untimely death of this son, who was a mathematician of considerable promise, he wrote to one of the early officers of the Mathematical Society, "My son's loss has thrown much grief over my house, but no gloom. He is spoken of as if he were removed to be followed in time, and to be kept in memory all the more, because he is no longer to remind us of himself by his direct presence. . . . I make out distinctly, from written evidence among his papers, that you and he were the projectors of the Mathematical Society . . . . and I wish to have the evidence of this very distinctly preserved. There is quite enough in my hands to establish the fact, but I should be glad of every detail that you can remember."

A very inadequate notion of Professor De Morgan will be formed by those who look only at his works. From them, indeed, it will be seen that he was a reader who relished every kind of intellectual food, and a thinker whose subtlety was only surpassed by his originality. They abound also with proof that he overflowed with humour; but his familiar associates alone can render justice to the versatility of his powers and the sweetness of his disposition.

Knowing many subjects thoroughly, there was scarcely one about which he did not know much.* He passed, for diversion's sake, from one arduous study to another, but though he found a pastime in intellectual efforts that would exhaust ordinary students, he did not disdain literature that pedants are apt to contemn as frivolous. He was an habitual and eager reader of novels, especially humorous novels, but there were times when, in the absence of a good novel, he could enjoy a bad one. The paradoxers, whom he infuriated by his banter, were strangely at fault when they accused him of malignity. He was the kindliest, as well as the most learned, of men—benignant to every one who approached him, never forgetting the claims which weakness has on strength. Soon after the death of his son the Professor was attacked with a disease of the kidneys, which during more than two years of distressing illness reduced him to a shadow of his former self; and on Saturday, the 18th of March, 1871, at one o'clock in the afternoon, his spirit was released from the body, which had long been only a burden to it.

* One of his favourite maxims was, "A man should know everything of *something*, and something of everything."

Lieut.-General Sir William Thomas Denison, K.C.B., R.E., was born in 1804. He was the son of John Denison, Esq., M.P., of Ossington, and brother of the Rt. Hon. John Evelyn Denison, M.P., Speaker of the House of Commons, and of the late Dr. Denison, Lord Bishop of Salisbury. After completing his preliminary studies, he entered the Corps of Royal Engineers, in which he was distinguished as one of the most successful of the scientific officers employed under the direction of Colonel Colby, Superintendent of the Trigonometrical Survey of Great Britain. While serving with his corps, he was frequently employed by Government on special engineering duties in England and the colonies. On Lieut. Denison's return from Canada, where he was engaged on the construction of the Rideau Canal, he was placed in charge of the course of Topographical instruction for Engineer Officers and Sappers at Chatham, in connexion with which he established an astronomical observatory. He subsequently had control of the works at Woolwich and Portsmouth Dockyards, employing his leisure time in editing the first series of Corps Papers on subjects connected with civil engineering. He became a Fellow of the Royal Astronomical Society in 1834, and of the Royal Society in 1838.

The administrative ability of Captain Denison having been brought under the notice of the Secretary of State for the Colonies, he was appointed, in 1846, to the office of Lieut.-Governor of Tasmania. About the same time he received the honour of knighthood. In 1854, he was removed to Sydney, as Governor-General of Australia, and finally he was translated in 1860 to the Governorship of the Madras Presidency. This latter tour of duty included a brief episode as acting Governor-General of India, during the few months' interval after the death of the Earl of Elgin in 1863, and the arrival of his successor Sir John Laird Mair (now Lord) Lawrence. Sir William Denison's brief administration of this high and responsible office gave complete satisfaction to the Home Government. He resigned his appointment as Governor of Madras in 1866, and returned to England in May of that year, thus closing a successful career as a Colonial Governor for the long period of twenty years.

While at Sydney and Madras Sir William Denison took a great interest in the promotion of astronomical observations in connexion with the established observatories at these places, and he was ever ready to support anything which he considered advantageous towards the progress of Astronomy. During his long absence from England he never failed to carry on a frequent correspondence on astronomical and other scientific questions with some of the principal philosophical minds of this country, by some of whom he was much esteemed.

Sir William Denison married, in 1838, Caroline Lucy, daughter of Admiral Sir Phipps Hornby. He was made a Knight Companion of the Bath in 1856, a Major-General in

March 1868, and a Lieut.-General in the latter part of 1870. After his return to England, he employed his leisure time in the preparation of his most characteristic work, *Twenty Years of Vice-Regal Life;* and after a few years of arduous application to the duties of a River Commissioner, as well as much attention bestowed on colonisation, he was called hence, after a very brief illness, on the 19th of January, 1871, in his 67th year, in all the honours of deep and unfeigned attachment amongst his personal friends; and amongst those who only knew him in public life, with all the respect due to his unblemished career as a "soldier, an officer, and a gentleman,"—such at least is the testimony of a few brother officers who have been privileged with his friendship during half a century.

EDWIN RICHARD WINDHAM WYNDHAM QUIN, Earl of DUNRAVEN and MOUNT EARL, was born at Adare Manor, in the county of Limerick, on the 19th of May, 1812. He was Lord Lieutenant and Custos Rotulorum of the County of Limerick, Knight of the order of St. Patrick, and a Commissioner of National Education in Ireland. From the year 1837 to 1851 he represented Glamorganshire in Parliament. He was a man of singular accomplishments, and an earnest student of science.

Lord Dunraven was educated at Eton, and Trinity College, Dublin. He was at that time devoted to the science of astronomy, and made it an essential condition of his entering college that he should be prepared for it under the tuition of Sir William Hamilton, to whom he was introduced by his friend Dr. T. Romney Robinson, of Armagh. He resided for about two years at the Dublin Observatory, where he devoted himself earnestly to observing-work, and on one night alone he observed as many as 150 transits of stars. Such labour, however, proved injurious to his sight, the failure of which prevented him carrying out his intention of erecting at Adare a first-class observatory. His interest in the science, however, remained unabated. He became, in the year 1831, a Fellow of the Royal Astronomical Society. His visits to the late Lord Rosse and the late Mr. Cooper, of Markree, were sources of unmixed delight to him, as affording him the opportunity of resuming his favourite study; and towards the close of his life he watched with intense interest the construction of the great Melbourne telescope in Dublin. He was also an excellent practical geologist and botanist, and gifted with a singular appreciation of and intense enjoyment in Nature.

In the year 1840 Lord Dunraven formed the acquaintance of the late Dr. Petrie, and from this and the influence of the warm friendship which sprang up between them may be dated the commencement of his devotion to the science of archæology. He had watched the stoppage of the Ordnance Survey of Ireland with the same deep regret that was felt by all the enlightened men of the country, and in 1843 it was through his energy that a large and

remarkable meeting was held in London, at his house in Eaton Square, the result of which was that the then minister, Sir Robert Peel, consented to appoint a commission to take evidence and make a report on the entire question. This commission—consisting of Lord Dunraven (then Viscount Adare), Mr. Young (one of the Lords of the Treasury), and the Clerk of the Ordnance (Captain Boldero)—held their first meeting on the 14th of July, and presented their report in the following November; and among the multitudinous reports of commissions in reference to Ireland, it may be safely said that, whether the subject-matter or the character of the witnesses be considered, there are few others possessing so deep an interest. It does not deal with any political question, but with matters much more closely allied to the real interests of the country, and which come home in an especial manner to the minds of all who have her social welfare and material progress at heart.

In the year 1830 Lord Dunraven became a member of the Royal Irish Academy, of which he was afterwards a Vice-president and Member of Council. In 1834 he was elected a Fellow of the Royal Society, and in 1837 of the Royal Geographical Society. He was also a Fellow of the Geological Society, and a Member of the British Association for the Advancement of Science. He laboured with his usual energy in the formation of the Irish Archæological Society, founded in the year 1840, and the Celtic Society, founded in 1845; and to him were dedicated some very important works connected with the history of Ireland, such as the *Annals of the Four Masters*, translated and edited by O'Donovan; the *History of the Ancient Ecclesiastical Architecture of Ireland*, by Dr. Petrie; and the third volume of *Les Moines d'Occident*, by the Comte de Montalembert, which contains the life of St. Columba, and the history of the conversion of Ireland by the monks.

Lord Dunraven presided over the meeting of the Cambrian Archæological Association held at Bridgend, Glamorganshire, in 1869. The duties of the president at these annual meetings are always of an arduous nature, but with a courteous manner and a kindly disposition, combined with the accuracy of a local antiquary and the breadth of view and tolerance of a sound comparative archæologist, Lord Dunraven discharged these duties in the most admirable manner, both in the field and at his Welsh residence, Dunraven Castle, Glamorgan; and his address at the meeting was a well-studied epitome of the archæology of the district which was about to be explored.

In 1866, Lord Dunraven and some friends formed themselves into a committee for the sale of the museum collected by Dr. Petrie, who died in that year; and for the arrangement and publication of his posthumous works. At this time the idea suggested itself to him of completing the work left unfinished by Dr. Petrie, of the *History of the Ancient Ecclesiastical Architecture of Ire-*

land; and for four years afterwards he laboured hard and travelled through the country, photographing all the principal ruins, making measurements and ground-plans and descriptive notes for his book. While thus engaged his health broke down, and he never lived to accomplish the work commenced and carried on with so much fervour. In his will he has bequeathed a large sum towards the publication of this series of photographs, which will illustrate the ancient architecture of the country, commencing with the great forts of the pre-historic period, and forming a regular progression to the time of the Anglo-Norman invasion.

Lord Dunraven married twice. His first wife was Augusta, third daughter of Thomas Goold, Esq., Master in Chancery—a lady of singular talent and acquirements, a true musician and artist, while wise and full of charity in her domestic and social life. She died in 1867, and he afterwards married Anne, daughter of the late Henry Lambert, Esq., of Carnagh, who was M.P. for the county of Wexford in the year 1833.

For some time previous to his death Lord Dunraven's health had been breaking. At Malvern, on the 6th of October, 1871, he sank to rest, surrounded by his family, who watched him to the end.

Commander ROBERT WILLIAM HALE HARDY was one of the many naval officers who have from time to time shown an interest in astronomical pursuits. He entered the navy in 1806, as a second-class boy on board the *Ganges*, 74, Captain Peter Halkett. While serving, in 1810-11, in the *Caroline*, 36, Captain Christopher Cole, he assisted in the celebrated capture of Banda Neira, and of the Island of Java. He was also present at the siege of New Orleans in 1814. After his promotion to the rank of Lieutenant, on the 20th of February, 1815, he never served afloat. Commander Hardy joined the Royal Astronomical Society in 1849, and was a contributor on several occasions to the *Monthly Notices*. He was the possessor of a good telescope and some other astronomical instruments. He died at Bath in 1871, at an advanced age.

The name of HERSCHEL, mentioned in this the sadder portion of our anniversary proceedings, calls forth one of the best and deepest of our emotions, veneration for the memory of an illustrious man and faithful friend departed to his rest; but not departed before the imperishable labours of a long life had been nobly accomplished. From ourselves this tribute of respect is especially due, inasmuch as from the earliest rise of our Society until now that it has passed its fiftieth year, John Herschel promoted and adorned its progress in all kindly offices and with a constant solicitude. With the single exception of his early friend Babbage, who survived him but a few months, he was the last of that list of admirable men who launched the Royal Astronomical Society on its career of eminent

success and scientific usefulness. Happily their mantle has descended on successors still left to us, able and worthy to carry on the work which those men commenced so well. We have entered on the harvest of our fathers' labours, rejoicing in the not less successful efforts of their sons.

John Herschel was born at Slough, on March 7, 1792, in that house which his father's discoveries have rendered historical. A few traits of his boyhood, mentioned by himself in his maturer life, have been treasured up by those who were dear to him, and the record of some of them may satisfy a curiosity as pardonable as inevitable, which craves to learn through what early steps great men or great nations become illustrious. His home was singular,—and singularly calculated to nurture into greatness any child born as John Herschel was, with natural gifts capable of wide development. At the head of the house there was the aged, observant, reticent philosopher; and rarely far away, was his devoted sister, Caroline Herschel, whose labours and whose fame are still cognisable as a beneficent satellite to the brighter light of her illustrious brother. It was in the companionship of these remarkable persons, and under the shadow of his father's wonderful telescope, that John Herschel passed his boyish years. He saw them, in silent but ceaseless industry, busied about things which had no apparent concern with the world outside the walls of that well-known house, but which, at a later period of his life, he, with an unrivalled eloquence, taught his countrymen to appreciate as foremost among those living influences which best satisfy and elevate the noblest instincts of our nature. What sort of intercourse passed between the father and the boy, may be gathered from an incident or two which he narrated as having impressed themselves permanently on the memory of his youth. He once asked his father what he thought was the oldest of all things. The father replied, after the Socratic method, by putting another question, "And what do you yourself suppose is the oldest of all things?" The boy was not successful in his answers, whereon the old astronomer took up a small stone from the garden-walk: "There, my child, there is the oldest of all the things that I certainly know." On another occasion his father is said to have asked the boy, "What sort of things, do you think, are most alike?" The delicate, blue-eyed boy, after a short pause, replied, "The leaves of the same tree are most alike to each other." "Gather, then, a handful of leaves from that tree," rejoined the Philosopher, "and choose two that are alike." The boy failed; but he hid the lesson in his heart, and his thoughts were revealed after many days. These incidents may be trifles; nor should we record them here, had not John Herschel himself, though singularly reticent about his personal emotions, recorded them as having made a strong impression on his mind. Beyond all doubt, we can trace therein, first, that grasp and grouping of many things in one, implied in the stone as the oldest of things; and, secondly,

that fine and subtle discrimination of each thing out of many like things, as forming the main features which characterised the habit of our venerated friend's philosophy.

In due time he was sent to Eton. It was close to his home; too close, indeed, for the jealous inspection of a mother's eye, for it was not long before she saw him maltreated by a stronger boy, and he was removed from the great school. Perhaps happily so, for the atmosphere of the public schools of that day was not suited for the nurture of a philosophical astronomer. Thus he was educated at home; and how successful that education was is written in the volumes and the manners of his life; for John Herschel became every way an accomplished man; he was a good and a fond scholar, an excellent modern linguist, a musician, and accomplished in the literature of the day. He always spoke affectionately of his tutor Mr. Rogers, but that gentleman does not appear to have been more successful in his attempts to impart a knowledge of geometry to his illustrious pupil than are the majority of the tutors of the present day; for John Herschel has been known to state that he learned Euclid accurately and diligently, but that "he knew no more of its real bearing and intention than he knew of the man in the moon." This is a pity, but we have much reason to fear that this is a pitiably common result. Perhaps no one would be more amazed than the great Geometer of Alexandria himself, if he were to return to life and find that his books, written for mature philosophers, were put without note, comment, or preparation, into the hands of the boys of the nineteenth century, to flounder therein just as they may chance.

Be this as it may, Herschel was sent to Cambridge at the unusually early age of seventeen.

His University career would no doubt have been eminently successful to whatever college he might have been sent, but he must have found a great facility for the development of his natural bias in having been entered by his father at St. John's. That College was then, and has ever since been, conspicuous for the careful and judicious training and the generous encouragement of its younger members in every branch of academical learning, and we venture to say that in none was, or is, the *Principia* of Newton studied with greater intelligence or devotion. In Herschel's day, and for many subsequent years, this immortal work was presented to the students in the form of manuscripts, in which the prescribed sections of the book were provided in an English dress. Herschel, however, made it his duty to read nearly the whole book in the original Latin. This is one instance out of many others, of that thoroughness of work which he displayed through life; it was the bent and genius of his mind; it was the happy tradition of his home. With such natural gifts developed by so able a training, it is no wonder that Herschel graduated as the Senior Wrangler of his year. Peacock, afterwards Dean of Ely, a man of rare ability, was the

second on the list, followed by several others who became conspicuous in their day.

It is well known that great changes were imminent at Cambridge in the form of the study of mathematical learning at the time when Herschel was an undergraduate. From a variety of causes, not very easily understood, mathematical analysis had not made that advancement in England which had been so remarkable on the Continent ever since the death of Newton. The Cambridge professors and teachers acted as if they had a vested interest in the powerful geometry of the past; many of them were possessed of vast mental capacity, but they continued to encumber themselves with an old fluxional notation, and ponderous methods, which had long been discarded by the philosophers abroad. Waring at Cambridge and Lagrange at Turin were both born in the same year, 1736; both of them wrote—and wrote ably—on the same subject, the *Theory of Numerical Equations*, yet the difference between the notations and even the methods of the two analysts, is something incredible to a person who has never taken the trouble of making the comparison. But it was not Cambridge alone which was in fault; a similar remark is applicable to the writings of Thomas Simpson in London and Euler at St. Petersburg. Yet the two Englishmen were probably endowed with natural genius comparable with those of their foreign compeers.

It was Woodhouse at Cambridge who first applied himself seriously and systematically to remove this stigma and this obstacle to all true scientific progress. He paved the way to ultimate success, not so much by his deeper and more controversial treatises, as by an elementary work on Trigonometry, which for the first time opened the door of the Continental forms of analysis, to the English student. This able and important, though elementary treatise, was published in the year when Herschel joined the University, and the influence which it had upon his mind is evidenced by the fact that the first contributions which he made, and that too during the years of his undergraduateship, to a scientific periodical of the day, were based on what he had read in Woodhouse's book. It was natural also that Herschel, even before his degree, should warmly espouse and effectually assist the cause in which his senior was so actively engaged.

One of Herschel's first works after his degree was an effort in the same direction, namely, to render the magnificent methods and results of analysis, which had so long been familiar to the minds of foreigners, accessible to the English student. With this view, and in conjunction with two of his university friends, he translated the best elementary treatise of that day on the *Differential Calculus*, adding, thereto, a valuable appendix on the cognate subject of Finite Differences from his own hands. Not long after this, the same zealous mathematicians added a collection of examples and methods of the higher forms of analysis, which

utterly revolutionised the form of mathematical science, not at Cambridge alone, but throughout the country. In the parts added by Herschel and Babbage, that containing the finite differences by the former, and that containing a theory of functional equations by the latter, there was much that was striking and original. It was mainly by the writings of these eminent men, that the foundation was laid for that brilliant school of English analysts who are now, at least, the equals of their compeers on the Continent. It were invidious to name the living, but we may cherish a pardonable pride in being permitted to number two of this great school among the former Presidents of our Society, and to-day we hope to add a third to the list.

We have thus traced the career of Herschel up to, and just beyond, the time when he took his Bachelor's degree at Cambridge; and we shall merely add, that the authorities of the venerable College to whose fostering care he owed so much, cannot fail to read with peculiar pleasure the name of Herschel inscribed among the former fellows of their ancient house. Omitting here all notice of his earlier contributions to Nicholson's *Philosophical Journal*, and to the *Cambridge Analytical Society* of his own day, we come to his first communication to the Royal Society. It is dated from Slough, Oct. 6, 1812, and must therefore have been completed in the last long vacation of his undergraduateship, three months before his final examination for his degree. It bears the significant notice "*Communicated by William Herschel.*" It related to "Some Remarkable Applications of Cotes' Theorem," and was probably suggested by an elegant theorem of Vieta's, printed in *Woodhouse's Trigonometry*, but it proceeds far beyond any conception which seemed to have presented itself to Vieta's mind. In 1814 we find a second paper of his in the same Transactions, but it is now communicated in his own right as a Fellow of the Society. It is a paper of remarkable elegance and of subtle conception, entitled "Considerations of Various Points of Analysis;" he therein devotes himself to the development of generating functions, a branch of analytical mathematics which had successfully engaged the attention of Laplace. It is in this paper that he first discloses the true bent and fashion of his maturer mind; the natural fruit of that early direction of his thoughts indicated by the incidents of the stone and the leaves already narrated. He intimates that mathematical science having now made vast strides in the various details of its several distinctive branches, further progress was not now to be looked for in the direction of specific and isolated methods, but that the time was come when philosophers should look for some methods of expressing the results of their labours *as a whole*. He says, "Henceforth the attention of the scientific observer must be directed to those elevated stations from which distinct and extended views of the arrangement of this science *as a whole* can be obtained." It is this grouping of things together as a whole, which constituted an eminent characteristic of

Herschel's philosophy. With him it is not the age of this thing or of that thing, but the oldest of all things aged.

This paper is followed up by other communications on mathematical analysis, all written in the same spirit, and pursuing the same vein of inquiry. Herschel, however, quitted Cambridge soon after his degree, and placed himself in the hands of an eminent Chancery barrister in London with the view of qualifying himself for practice at the Bar. The arrangement in itself was natural enough, for the higher wranglers in the Cambridge tripos of Herschel's time, who selected the Law for their profession, almost universally rose to eminent stations among the chiefs in Chancery, or on the judicial bench. The names of Copley, Pollock, Bickersteth, Maule, Page Wood, and a host of others, bear witness to the happy results of the Cambridge training. And if a less amount of success has been attained by them in more recent times, whatever the other reasons may be, we venture to say that one of the efficient causes may be found in the modern comparative abandonment of the geometrical method. The educational effects of the continued study of abstract analysis do not easily comport with the composite circumstances of ordinary life, wherein lies the chief field for the application of the Law. Be this as it may, a mind constituted like Herschel's, and trained, as his had been trained, in the love of generalisation, was not likely to submit voluntarily to the mastery of isolated facts and precedents bound together apparently by no ties of a general law. Whether this was the cause or not, certain it is that he emancipated himself from studies which were essential to success at the English Bar.

Nevertheless, his removal from Cambridge to London proved to be the turning-point of his scientific career. For it was in London that he made his acquaintance with Wollaston and with South. Wollaston completely fascinated him by the breadth and accuracy of his scientific pursuits, and fostered, if not awakened, in him that dormant taste for Chemistry and general Physics which wanted but the magic touch of such a mind, to kindle into the intellectual passion of a life. We think we may aver this with safety, for we have heard him more than once say that he became an Astronomer, not so much from any conscious and specific aptitude for the work, as through a loyal desire to re-examine and complete his father's work: had he followed simply the natural bias of his mind, his pursuits would have been the study of Chemistry and of the Theory of Light.

Hence it is that in 1819 we find him communicating to Nicholson's *Philosophical Journal* a very remarkable article on Hyposulphurous Acid and its compounds. He therein details the important and indeed the cardinal discovery of that property of hyposulphite of soda, which, had it been known to Wedgwood or Davy, would have anticipated the photographic invention of Neipce and Daguerre by a quarter of a century. "Muriate of silver freshly precipitated," he says, "dissolves in this salt (hyposulphite

of soda) when in somewhat concentrated solution, in large quantity, and almost as readily as sugar in water." What would Davy have given for the knowledge of this discovery, when he shut up his Sun-pictures in the darkness of a portfolio, only to elude him like ghosts in daylight? This was immediately followed by his first paper on physical optics, a subject which, as we have said, had a peculiar attraction for him; and here again we find an instance of his love of generalisation, for he applies the known action of grooved or striated surfaces on light to explain the iridescence of mother-of-pearl. Then, again, in the same year, there is a communication made to the Royal Society, on another optical question, much more recondite in its nature: he investigates the coloured lemniscates produced by certain biaxal crystals, when submitted to the passage of polarised light, and he proposes certain simple and elegant devices for the observation of these and similar gorgeous phenomena sometimes adopted even at the present day.

Lastly, there came a very important paper addressed to the Royal Society on the subject of aplanatic combinations for the object-glass of a telescope. There is great power and mathematical elegance in the management of the symbols, and the way in which he proposes to employ one of the disposable constants, so as to make the combination equally aplanatic within a considerable range of distance, is ingenious in the extreme. Practical opticians, however, have not generally availed themselves of this form, because other forms are of less difficult execution. Moreover, now that the manufacture of glass admits of apertures which Herschel never dreamed of, it is found that, however useful mathematical formulæ may be for a first approximation to the proper form of the components of a large object-glass, nevertheless when this approximation has been executed, the achromatism requires a subsequent manipulation, often expressed by the significant term "finishing," which must be effected by the skill and experience of the artist, rather than from abstract calculation. Still Herschel's investigation is effective for moderate apertures, and for a camera applied to landscapes might be very beautiful.

It was about this time also that the influence of Mr. South produced a decided effect upon his pursuits. That gentleman was possessed of some of the best achromatic telescopes of the day, small as they were compared with the gigantic machines of more modern times. He had a passion for practical Astronomy, and had by himself done some very creditable work on double stars. It was a happy circumstance for him and for science that he could be associated with such a mind as Herschel's. Their conjoint labours commenced about the same time as the first formation of our own Society. South himself having a large share in the scheme which has been so successfully developed during the last fifty years. The venerable Sir William Herschel was its first President, and his illustrious son its first Foreign Secretary. The early lessons in the historic house had been well learnt, and

the teacher was well rewarded, by this happy and significant association.

Herschel's first communications to the Astronomical Society were efforts made to relieve practical astronomers in certain numerical calculations at once intricate and laborious. The first related to the occultations of fixed stars by the Moon; and the second to the arrangement of tables for the reduction of certain stars to their mean places at an assigned epoch. The first involves mainly geometrical considerations; but to handle the latter with any success, requires all the resources of a knowledge of some of the most intricate questions in astronomical dynamics.

From 1821 to 1823 he was associated with Mr. South in Blackman Street, Southwark, in the reobservation of the double stars discovered by his father. He had himself commenced the work as early as 1816, deeply interested in those marvellous movements of Sun around Sun which had been brought to light by the labours at Slough. He now resumed them in London with instruments in Mr. South's possession, described by Mr. Baily, the President of our Society in 1826, as "a princely collection, of exquisite workmanship, and considerable magnitude, such as have never yet fallen to the lot of a private individual." The result of their joint labours is printed in the Transactions of the Royal Society, and their importance and success were acknowledged by medals from that learned body, and subsequently from our own, and by the Lalande Medal of the French Institute. It is a matter of scientific interest to observe that a similar acknowledgment was at the same time made by our Society to Wilhelm Struve for the first-fruits of similar labours, undertaken with Fraunhofer's far-famed achromatic telescope at Dorpat. To this work Struve says he was fired by the noble example of William Herschel.

The conjoint labours of the two astronomers came to an end by the removal of South to the neighbourhood of Paris, in search of a clearer atmosphere than that which enveloped our own metropolis even half a century ago; the result was pretty much the same as that experienced subsequently by other observers, namely, that he would have been equally successful at home. Meanwhile, and after the termination of his travels on the Continent of Europe, Herschel resumed at Slough the dutiful but self-imposed and gigantic task of re-examining the double stars and nebulæ which through a long life had formed the solicitude of his illustrious father. It is interesting to read his own description of the instruments employed, he says,—

"The telescope with which the following observations were made is one of that construction which has been called the 'front-view.' The aperture, or clear polished surface of its mirror, is 18 inches, and its focal length 20 feet. It was constructed in the year 1820, under the joint superintendence of my father and myself, on the model of that used by him in his *Sweeps of the Heavens*, whose place it was intended to supply, the woodwork of

the latter being greatly decayed by age. At the same time a new speculum was cast, with a view to preserve the old one, the figure of which was remarkably fine for the most delicate observations." And then at a subsequent part of his *Introduction* he proceeds to say: "It will now be right to mention more particularly the modifications I have introduced into my father's system of sweeping, —modifications rendered necessary by the loss of my aunt, Miss Caroline Herschel's personal assistance, on whom the task of reading and registering the Polar Distance and Right Ascensions of objects, writing down the remarks and descriptions, warning the observer of expected stars, and finally reducing and calculating the whole, used invariably to devolve. Unsupported by such aid, I am under the necessity of recording the observations myself; an inconvenience of the worst kind, not only as it diminishes by at least one-half the number of the objects that can be taken, but because the frequent admission of extraneous light to the eye is fatal to observations of the fainter nebulæ."

These labours extended as far as a Sixth Catalogue, which was printed in the ninth volume of the *Memoirs of the Royal Astronomical Society* in 1835. Meanwhile observations of the nebulæ visible in these northern regions were proceeding steadily; but these were reserved for the *Transactions of the Royal Society*, with the exception of a most interesting and admirable monograph on the Nebulæ of *Orion* and *Andromeda*, which will be found in the second volume of our own. We would earnestly recommend those who are interested in the history and progress of science, and especially Astronomical science, to read the introduction to such memoirs as these of the two Herschels, and of other great Astronomers of half a century back, among many other cogent reasons, especially in order to know how deeply modern observers are indebted to the ingenuity of their predecessors. Through their persevering ingenuity we have been gradually provided with instruments and methods which save an amount of labour and of anxious manipulation which appears incredible when understood. The smooth automatic motion of the magnificent astronomical machines of modern days, leaving both hands at liberty for adjusting the exquisite micrometers provided by the great artists of our day, will be all the better appreciated if they will master and reflect on the comparatively clumsy, harassing, and laborious, though efficient, expedients necessarily adopted by the two Herschels. It seems indeed to be an inevitable law imposed on the achievement of all great work, and on all great discoveries, that they shall be completed under disadvantages. Neither Dalton nor Davy nor Wollaston nor Faraday luxuriated in the appliances at this day found in our physical laboratories.*

* In connexion with this, and with the amazing advances recently made in the delicate accuracy of modern mechanical engineering, we may here mention an anecdote not without its interest. Some time ago we ventured to assert that the above mentioned improvements were mainly due to Mr. Babbage's attempts to complete his calculating machine, and we wrote to Mr. Babbage on the subject.

In 1827 we find John Herschel occupying the chair of the Royal Astronomical Society; and now commenced that series of presidential addresses, which we do not hesitate to say are unrivalled for instructive eloquence, masterly arrangement, and comprehensiveness of view. We shall here give one specimen, taken from the address which he delivered on presenting his friend, Baily, with the Society's medal for the completion of his superintendence of the Society's Catalogue of Stars. It would seem almost impossible for any imagination, however vivid, to clothe the description of so bare a thing as a catalogue of stars, with the amenities of human interest, and the graces of a poetic eloquence. Yet it will be seen that Herschel has accomplished this feat; and, probably, without the consciousness of attempt. He describes what, as an accomplished philosophical astronomer, he felt—and no doubt had often felt before. "If we ask to what end magnificent establishments are maintained by states and sovereigns, furnished with master-pieces of art, and placed under the direction of men of first-rate talent, and high-minded enthusiasm, sought out for those qualities among the foremost in the ranks of science: —if we demand *cui bono?* for what good a Bradley has toiled, or a Maskelyne, or a Piazzi worn out his venerable age in watching? the answer is, not to settle mere speculative points in the doctrine of the universe; not to cater for the pride of man, by refined inquiries into the remoter mysteries of nature,—to trace the path of our system through infinite space, or its history through past and future eternities. These indeed are noble ends, and which I am far from any thought of depreciating; the mind swells in their contemplation, and attains in their pursuit, an expansion and a hardihood which fit it for the boldest enterprise.—But the direct practical utility of such labours is fully worthy of their speculative grandeur. The stars are the land-marks of the universe; and amidst the endless and complicated fluctuations of our system, seem placed by its Creator as guides and records, not merely to elevate our minds by the contemplation of what is vast, but to teach us to direct our actions by reference to what is immutable in His works. It is, indeed, hardly possible to over-appreciate their value in this point of view. Every well-determined star, from the moment its place is registered, becomes to the astronomer, the geographer, the navigator, the surveyor,—a point of departure which can never deceive or fail him,—the same for ever and for all places, of a delicacy so extreme as to be a test for every instrument yet invented by man, yet equally adapted for the most ordinary purposes; as available for regulating a town clock, as for conducting a navy to the Indies; as effective for mapping down the intricacies of a petty barony, as for adjusting the boundaries of transatlantic empires. When once its place has been thoroughly ascertained and carefully recorded, the brazen circle, with which

His reply was, "You are quite right; it used to be said by the men in my workshop, Mr. Babbage made Clement, Clement made Whitworth, and Whitworth made the tools."

that useful work was done, may moulder, the marble pillar totter on its base, and the astronomer himself survive only in the gratitude of his posterity: but the record remains, and transfuses all its own exactness into every determination which takes it for a ground-work, giving to inferior instruments, nay even to temporary contrivances, and to the observations of a few weeks or days, all the precision attained originally at the cost of so much time, labour, and expense."

We enter now upon a summary of the labours, which this earnest philosopher prosecuted with undeviating diligence, during his sojourn at the Cape of Good Hope. We should have called this the most remarkable period of his life (and we have heard those who knew him best speak of it as the happiest), but, whether he is at Cambridge, or at Slough, or at Feldhausen, or at Collingwood, each period of that busy existence carries with it its own distinctive labours, and is full of its own philosophic interest.

The motives which impelled him to take this distant journey, cannot be better conveyed than in his own language, which we transcribe from the volume containing the results of his labours and his scientific musings in South Africa. He states that about the year 1825 he had commenced a re-examination of various observations which had occupied the greater part of his father's life; he then rearranged the whole, and communicated the results from time to time to the Royal, and Royal Astronomical Societies. He then goes on to say, "Having so far succeeded to my wish (the places of the objects thus determined proving on the whole satisfactory), and having by this practice acquired sufficient mastery of the instrument employed (a reflecting telescope of 18¼ inches clear aperture and 20 feet focus, on my father's construction), and of the delicate process of polishing the specula; being, moreover, strongly incited by the peculiar interest of the subject, and of the wonderful nature of the objects which presented themselves in the course of its prosecution, I resolved to attempt the completion of a survey of the whole surface of the heavens, and for this purpose to transport into the other hemisphere the same instrument which had been employed in this, so as to give a unity to the results of both portions of the survey, and to render them comparable with each other.

"Accordingly, having placed the instrument in question, as well as an equatoreally mounted achromatic telescope of 5 inches aperture and 7 feet focal length, by Tulley, which had served me for the measurement of double stars in England, together with such other astronomical apparatus as I possessed, in a fitting condition for work, and taking every precaution, by secure packing, to insure their secure arrival in an effective state, at their destination, they were conveyed (principally by water-carriage) to London, and there shipped on board the *Mount Stewart Elphinstone*, an East India Company's ship, in which, having taken passage for myself and family for the Cape of Good Hope, we

joined company at Portsmouth, and sailing thence on 13th November, 1833, arrived, by the blessing of Providence, safely in Table Bay on the 15th January, 1834." He then proceeds to explain the position of the residence he was fortunate enough to acquire by purchase, and adds that all preparations were "pushed forward with such effect, that, on the 22nd of February, I was enabled to gratify my curiosity by a view of α *Crucis*, the Nebula about η *Argûs*, and some other remarkable objects, in the 20-feet reflector; and on the night of the 5th of March to commence a regular course of sweeping."

The vast mass of scientific labour completed during the next four busy years at Feldhausen may be properly described by giving a very rapid summary of the principal contents of the volume of the *Cape Observations;* a work which we venture to say is not surpassed in varied interest or importance by any astronomical work in existence. It was not published until 1847, nine years after the author's return to England, for the very cogent reasons assigned by himself: "The whole of the observations, as well as the entire work of reducing, arranging, and preparing them for the press, has been executed by myself." Perhaps none but practical astronomers can form a true conception of the incredible amount of personal and tedious labour implied by these words. Nine years, indeed, would not have been sufficient to complete the task, had not the work been first planned with a careful sagacity, and then carried out with undeviating perseverance.

The first chapter of the volume, consisting of 164 quarto pages, records the history of his observations of the Southern Nebulæ and clusters of stars. They are then systematically arranged in a catalogue, containing the places of 1707 of these objects, all reduced to the epoch 1830, and each object bearing, in condensed literal symbols, a complete description. But this gives a very inadequate view of his labours among the Southern Nebulæ. He selects the most remarkable objects among them, delineates them with a scrupulous care, and then makes charts and catalogues of the numerous small stars in their neighbourhood, with the view of the future detection of any changes in these mysterious bodies. Completeness and utility are evidently the aims which direct and sustain his labours. His delineations and descriptions of the great Nebula of *Orion*, and of the region surrounding the remarkable star η *Argûs*, will long remain among the most remarkable of astronomical monographs; and the latter is at this moment, nearly thirty years after its publication, of singular importance, as serving to establish the present non-variableness in the form of the adjacent Nebula, which has become a subject of dispute. The labour and the thought expended upon these detached and selected objects, probably equals that required for the observation of all the other Nebulæ combined. In a minute portion of the heavens, scarcely exceeding twice the space occupied by the Moon's disk, there are recorded the places of no less than 1216 stars. A spangle viewed

at arm's length would eclipse the whole; yet he himself acknowledges it was the anxious work of several months.

These catalogues and charts are no sooner completed than he proceeds to discuss questions connected with them, and of the most absorbing interest. He inquires into the law of the distribution of these wonderful masses of glowing vapours, and these clusters of luminous suns. He thereby confirms his father's hypothesis that the Nebulæ are not irregularly scattered, and with no apparent law, hither and thither in the visible heavens, but are collected in a sort of canopy in the vertex or pole of that vast stratum of stars in which our Solar System finds its own position, buried in it, as he supposes, at a depth not greater than that of the average distance of an eleventh-magnitude star. These remarkable conclusions of the two Herschels are now being subjected to a re-examination by Mr. Proctor; but whatever the result may be, it is to the astounding industry and sagacity of the two Herschels alone, that so magnificent a conception has been entertained and brought into the cognizance of human thought.

The Nebulæ being at length dismissed, he proceeds to detail his observations of the double stars which had presented themselves during the sweeps for nebulæ. They are not dismissed until a regularly arranged catalogue of all that he had observed was completed, and ready for comparison by any subsequent astronomer. But there was a point of view which possessed for Herschel a peculiar charm. His father, half a century before, had commenced the observation of these singular objects—singular from their optical proximity, and still more so from the contrast of the colours of so many of these binary combinations. The motive which had invited the elder Herschel to the work, was a well-grounded hope that they might lead to the discovery of the distances of many stars from the Earth; this was the one great element which was wanting for the completion of his magnificent conception of the distribution of the universe of stars. He failed in this, but his failure herein was at length accompanied by the discovery that many of these stars moved round each other, and were to each other as revolving suns. John Herschel also had invented an elegant and practicable method of determining the forms of these orbits, and the periods which these suns take to complete their revolutions. In particular he had calculated the orbits of the two stars which form the combination γ *Virginis*, and he had the crowning satisfaction of seeing what he terms their *perihelion appulse;* he saw the two stars (as he had predicted) so close together, that his telescope was unable to detect their duality. Such visible reward of honest labour is not always accorded to the labourer. At the same time Herschel took the opportunity of improving certain imperfections which still adhered to his method of treatment. The period of revolution he ascertained to be approximately 182 years, not greatly exceeding the period of the planet *Neptune* round our Sun. If *Neptune* had been greatly larger than he is, would he have now shone by other than a reflected

light? And if so, what sort of dynamical problem would the perturbation of our Earth and Moon have presented to the mathematical Astronomer?

In the course of the chapter which contains the catalogue and investigations of the double stars, Herschel makes one of those characteristic remarks which, while they enliven the narrative serve for us a still higher purpose; they exhibit the perseverance and the self-denial of the observer. He is referring to certain small regions of the heavens, which are barren of interesting objects, and he observes that it was well for himself to enter some remarks in his note-book "for the homely, but useful purpose of avoiding sleep,* and that not unattended with the probability of broken bones." The well-known picture of the mounting of the Herschelian Telescope, will recur to the memory of our readers, and they will readily imagine that a fall from the observer's subaërial stage was by no means impossible, and assuredly dangerous if it occurred. It is by the path of labour and self-denial, that to great men is accorded the meed of immortality by their fellows.

The double stars and their stately revolutions and lustrous colours dismissed, in the next chapter he proceeds to describe the methods by which the magnitudes, or rather the relative brightness of the stars, may be observed and catalogued. In the library of the Astronomical Society there are preserved certain sheets of paper, on which he had marked the allineations and magnitudes of all the stars visible to his naked eye in the Northern hemisphere. The labour is less than it appears to be, owing to the comparatively small number of stars visible to unaided vision at one place; John Herschel accomplished it for the stars in both hemispheres. And if the detail of such occupation shall perchance raise the question whether such labour is misplaced for any man, and still more so for such a man as Herschel, let us listen to his own account of it.

"The subject," he says, "is one of the utmost physical interest. The grand phenomena of geology afford, as it seems to me, the highest presumptive evidence of changes in the *general* climate of our globe. I cannot otherwise understand alternations of heat and cold, so extensive at one epoch as to have clothed high Northern latitudes with a more than tropical luxuriance of vegetation; at another to have buried vast tracts of Middle Europe, now enjoying a genial climate, and smiling with fertility, under

* John Stone, the worthy mechanic who assisted Herschel at the Cape, and of whom he makes honourable mention in the preface to his book, informed the writer that on one occasion when Herschel was observing Halley's comet, the astronomer *did* actually fall asleep. It was Stone's office to *say* nothing, but to turn the telescope. He kept at his post, but wondered at his master's long silence, and in due time he turned the telescope in the direction of the Table Mountain, Halley's Comet being now behind it; on a sudden Herschel awoke, and the next moment looking into the telescope, exclaimed, "Stone! Stone! where *has* the Comet gone to?"

a glacier crust of enormous thickness. Such changes seem to point to some cause more powerful than the mere local distribution of land and water (according to Mr. Lyell's views) can well be supposed to have been. In the slow secular variations of our supply of light and heat from the Sun, which in the immensity of time past may have gone to any extent, and succeeded each other in any order, without violating the analogy of sidereal phenomena which we know to have taken place, we have a cause, not indeed established as a fact, but readily admissible as something beyond a bare possibility, fully adequate to the utmost requirements of geology. A change of half a magnitude in the lustre of the Sun, regarded as a fixed star, spread over successive geological epochs, —now progressive, now receding, now stationary, according to the evidence of warmer or colder general temperature which geological research has disclosed, or may hereafter reveal,—is what no Astronomer would now hesitate to admit as in itself a perfectly reasonable and not improbable supposition. Such a supposition has assuredly far less of extravagance about it than the idea that the Sun, by its own proper motion, may, in indefinite ages past, have traversed regions so crowded with stars as to affect the climate of our planet by the influence of their radiations."

Such, then, was one of the reasons which actuated both the Herschels in their midnight survey of the stars; and we may, in passing, mention another instance of J. Herschel's methodical diligence in this, that he had proposed to himself to devote the enforced leisure of the voyage home by sea, to the completion of these comparisons of stellar brightnesses, and so carry on the sequence of these relations to the stars of the north, not visible in South Africa. Bad weather, however, interfered with the consecutiveness of the work, and two nights only were favourable to his design. He says:

"It was my earnest wish to have carried on these comparisons during the whole of our homeward voyage (the observations being easily made on ship-board) so as to interweave in sequences sufficiently numerous and extensive, at least all the most conspicuous stars of both hemispheres; but in this I was disappointed, at least to that extent: as a totally overcast state of the sky prevailed from the day of our departure from Table Bay, until we reached nearly the latitude of St. Helena, with the exception of a single fine night on the Equator, on the 28th of March, 1838, advantage of which was taken to procure a short sequence, and several valuable comparisons of Southern stars with stars of equal or nearly equal lustre in good situations for European observations. But, with this exception, no other available opportunity occurred till the 14th and 15th of April, when two sequences were observed in latitude 17° and 18° north; but in these the more Southern stars were already too low for fair comparison."

After the discussion of the magnitudes and distribution of the stars in the Southern hemisphere, there comes a chapter upon Halley's Comet, containing many interesting observations on the

physical features and dynamical action of cometary matter. He lived long enough to learn how great a stride has been made in our knowledge of such things, by the combined labours of such men as Kirchhoff, Huggins, and Schiaparelli; nevertheless, Herschel's discussion is replete with instruction. Similar remarks may be made with respect to his discussion of Sun-spots, and this is rendered all the more valuable from the impetus which it gave to Mr. De La Rue, Prof. Selwyn, and others, whereby they were induced to commence their invaluable labours on the Sun's autography. It is in this chapter that he suggests that form of first-surface reflecting eye-piece, which has completely revolutionised the method of viewing the Sun's photosphere.

Such is an outline, and a bare and imperfect outline, of the contents of this impressive volume. In perusing it we can with difficulty persuade ourselves that in reality it comprises the chief labours of four years only of a philosopher's life. Happily John Herschel, in this respect of devoted and intelligent labour, does not stand alone among the other great Astronomers of his age. The world hears but little of them at the time; they neither strive nor cry in the streets, but their labours remain as imperishable as the genius which inspires them.

As was natural and honourable to all concerned, he was greeted with enthusiasm on his return to England. There is something contagious in a great example, and one of Herschel's rewards consisted in this, that he inspired others with a zeal kindred to his own. In due time another Astronomer, Mr. Lassell, at his own expense, and with a more magnificent instrument, constructed like Herschel's with his own hands, expatriated himself and his family to Malta for three years, under the hope of adding, if possible, to the discoveries of Herschel. In this he succeeded. Herschel must have had multitudinous thoughts pressing on his mind when, in 1849, he placed the Gold Medal of the Astronomical Society in Mr. Lassell's hands, saying, that "the simple facts are these: Mr. Lassell cast his own mirror (24 inches in diameter), polished it by machinery of his own contrivance, mounted it equatoreally in his own fashion, and placed it in an observatory of his own engineering. With this instrument he discovered the satellite of *Neptune*, the eighth satellite of *Saturn*, and reobserved the satellites of *Uranus*."* He spoke, moreover, of Mr. Lassell as one of those, "who, by their personal example, press forward the advent of that higher phase of civilisation which some fancy may see not indistinctly dawning around them; a civilisation founded on the general and practical recognition of the superiority of the pleasures of mind over those of sense; a civilisation which may dispense with luxury and splendour, but not with the continual and rapid progress of know-

* Mr. Lassell did more than this subsequently at Malta. He in reality discovered two satellites of *Uranus*, and rendered it exceedingly probable that four satellites only have yet been seen, instead of the six suspected by the elder Herschel.

ledge in science and excellence in art." In mentioning Mr. Lassell, we gladly also recall to our memory similar scarcely less important labours of Mr. De La Rue.

In 1830 appeared his well-known *Preliminary Discourse on the Study of Natural Philosophy*; this was followed by a similar volume on Astronomy, both being published in *Lardner's Cyclopedia*. The latter, in 1849, was superseded by his larger and more important *Outlines of Astronomy*. No comment of ours is needful on these remarkable books; any one of the three would have sufficed to establish the scientific reputation of an ordinary writer. The first of them may be regarded as having contributed in no small measure to the fostering of that taste for scientific pursuits which has lately become a characteristic of our age. There is a sweet and dignified persuasion animating the volume, which, to a reader with a well-constituted mind, is irresistible. The last of the three, like the first, has become a classical work in our language; and has been the means of directing many hundreds of persons to the pursuit of a science which he knew to be inferior to none in its humanizing tendencies. These books are nothing short of a benefaction to mankind.

There are some passages in the former of these volumes which are displeasing to the disciples or admirers of Comte. Herschel, in all loyalty to his own science, had exposed certain shallow conceptions and scientific blunders of that talented but singular man; and he had no sympathy with his philosophy. He had a profound belief in causation as distinct from sequence, and in a great First Cause, and in many passages of his writings he had traced the evidence of a Personal Will in nature. Hence Herschel was not forgiven by a certain class of metaphysical writers. They were his sole literary antagonists; personal enemies he had none.

His two treatises on *Light* and *Sound* were published in the *Encyclopedia Metropolitana* about the year 1830. The former has probably been read by every competent mathematician in the country, and every reader will have felt the charm of the half-suppressed enthusiasm which has carried him along. It contains the results of his own experimental investigations on the action of certain crystals on polarized light,—plagiedral quartz, apophyllite, Rochelle-salt, for instance,—results which have been confirmed and generalized since his early days. It would be a national advantage in the education of our higher classes of mathematical students if authors would take a leaf out of Herschel's books, and clothe the dry bones of their writings with a little judicious historical or experimental illustration.

But there is another volume of his not so widely known to the public as either of the former works. In this volume he collected many of his addresses to public bodies,—in particular, to the Royal Astronomical Society, and to the British Association. It was published in 1857 by Longmans. We venture to say it is [illegible] that the great philosopher is seen in his happiest and most

instructive mood, for these addresses contain the outpourings of a man walking at liberty among sympathizing associates, in his own home, and enamoured with its interest and beauties. Whether it is the affectionate yet wise earnestness with which he dilates on the comprehensive labours of a Bessel, or on the ponderous analysis of a Plana, or on the intelligent and loyal devotion of his friend, Baily, to the science and the society of his adoption, all teem with historical associations and pregnant suggestions which will richly reward the student who will devote his hours to their perusal.

In this volume, his review of *Quetelet on Probabilities* is reprinted; it favourably exhibits Herschel in the guise of a social philosopher, and it contains by far the clearest popular exposition of the principle of Least Squares that has yet appeared. An able and very recent writer on this difficult subject in the *Transactions of the Royal Society*, ascribes to this demonstration a character higher than popular.*

The volume terminates with a collection of certain of his efforts in fugitive poetry. For John Herschel was a poet in a high sense, and of science he was emphatically *the* poet. Without imagination no man can become a truly great philosopher, and still less a truly great writer; and Herschel was both; and hence with him, as with many other of our ablest mathematicians, versifying became a solace and a recreation from severer work. There are passages in some of these little poems, and there are lines in his more formal translation of the *Iliad*, published at a much later period, which plainly indicate that had he chosen to make poetry the occupation of his life, he must have occupied a conspicuous place among the poets of his age. His sonnet on Sir William Hamilton in Ely Cathedral, and his verses entitled "A Dream which was not all a Dream," and which furnished one of his solaces in his latest hours, are sufficient evidence of the estimate we have made. Nevertheless it seems astonishing that a man with so cultivated an ear for music as he possessed, should have encumbered himself with the impossibilities of English hexameters, not feeling that the sweet and varied cadence of the Homeric metre cannot be transfused into our less flexible language. It is here, perhaps, that we may mention, as a pleasing trait of the

* In this volume is included a very able and interesting review of Mrs. Somerville's *Mechanism of the Heavens*, in which he reverts to what occurred at Cambridge during the transition of the mathematical studies of the place, to which we have referred in page 125 of this memoir. He says:—"Students at our universities, fettered by no prejudices, entangled by no habits, and excited by the ardour and emulation of youth, had heard of the existence of masses of knowledge from which they were debarred by the mere accident of position. These required no more. The prestige which magnifies what is unknown, and the attractions inherent in what is forbidden, coincided in their impulse. The books were procured, and read, and produced their natural effects. The brows of many a Cambridge Moderator were elevated, half in ire, half in admiration, at the unusual answers which began to appear in examination papers. Even Moderators are not made of impenetrable stuff: their souls were touched, though fenced with seven-fold Jacquier, and tough bull-hide of Vince and Wood."

habits of his life, that one of his last letters was written to his friend Sir Thomas Maclear at the Cape, and contained some popular nursery rhymes translated into Latin verse.

It is instructive to observe what sort of work Sir John Herschel reserved as the occupation of his old age. He could hardly expect at the age of seventy to make any positive advances in the science which had already occupied so many years of an active life. Besides the *Translation of the Iliad*, already referred to, he busied himself in arranging his own and his father's observations in a complete and collected form; he wrote, also, several very striking articles on some of the more popular aspects of science, in *Good Words*. These and a few other lectures and addresses he collected in a single volume, bearing the title *Familiar Lectures*,—a little book, but eminently calculated to diffuse a taste for the study of the forces in constant operation around us; and in many of its paragraphs so suggestive as to arrest the attention even of those who are proficients in the subjects on which he writes. We entertain no doubt that the motive which impelled him to write this book was that which he had so well expressed in his address to the Educational Institute in South Africa, and which we have already quoted, namely, the importance which he attached to the diffusion of scientific knowledge, as a civilising agent, among the great masses of the people; for it was a characteristic of Herschel, that what he wrote, he wrote with settled purpose long entertained, and we may here be permitted to record an instance of this predetermination. We had occasion, a few years ago, to write to him to request a reference to some of the many passages in his writings wherein he had referred to the evidences of the action of a Personal Will in nature. The immediate reply was a little manuscript containing the transcription of all the passages which had been the object of our search, and which he had himself collected for his own purposes long before. A week or two before his death he again wrote with some solicitude on the same subject. This conscious and pervading intention of his life, is rendered still more evident by the state of completeness in which he left the record of all the most important undertakings of his scientific career. Thus, seven years before his removal, and therefore in his seventy-second year, he presented the Royal Society with a magnificent catalogue of all the nebulæ that had been recognised up to the date of its completion, whether observed by himself, by his father, or by any other astronomer.

Hence, again, during the last few years of his life he busied himself in doing what perhaps no other man could at any time have done so well, namely, in arranging a catalogue of all the double stars that were known to exist up to the most recent date. This catalogue was intended to contain what may be termed the natural history of every such star; that is to say, everything that had been observed or known concerning it. This grand and gigantic work was arranged and completed just

before his death, so far as the catalogue itself is concerned. It contains about 10,000 double stars, all arranged in Right Ascension and Polar Distance, and the entire history of about 5000 of these stars is completed. He bequeathed this precious monument of his intelligent labour to the Astronomical Society; and we, to whom the editing is confided, shall most assuredly write with a loving and a truthful pen, on the last page of the volume the motto which in life pleased him so well, "CŒLIS EXPLORATIS."

John Herschel met with an amount of public recognition unusual in England. He might have been, had he wished it, President of the Royal Society, and he four times accepted the chair of the Royal Astronomical Society; the Society pleased him in its unpretendingness and solid work. He was appointed to the Mastership of the Mint, as Newton had been before him; but he fell upon the evil days of a reform in the place, and his sensitiveness was, in consequence, put to many a severe trial. He held the office for five years. At an early period of his life he received the recognition of a Hanoverian Knighthood; and at a later period the heritable title of a Baronetcy was accorded to him and his family. The scant measure of even this reward, raises difficult questions in the mind, and great searchings of heart. Naturally he was a member of almost every important learned society in either hemisphere. The French Institute honoured themselves by at last electing him a Foreign Member of their body. He married Margaret Brodie, daughter of Dr. Stewart, in 1829: she and a numerous family survive him. Two of his sons are already very favourably known in the realm of science, and their father lived to see one of them selected by the Council for election to the Fellowship of the Royal Society. It would have been a gratification to him, had he lived long enough, to see the other appointed to a very important Professorship in the North of England.

What was Herschel's true place in the philosophy and among the other great lights of his age, cannot be accurately fixed until his own generation shall have entirely passed away; the partialities and the prejudices of contemporaneous life, unavoidably warp and incapacitate the judgment, just as too close a proximity to a mass or a multitude is unfavourable to a correct appreciation of its proportions. The judgment passed by the able writer of his obituary notice in the *Proceedings* of the Royal Society, is naturally influenced by an affectionate remembrance of his friend, nevertheless it is not unsupported by less partial critics: he says, "British science has sustained a loss greater than any which it has suffered since the death of Newton." Long ago a somewhat similar estimate of his powers, both intellectual and moral, was made by one of the most competent among foreign *savans*, the celebrated Biot, to the effect that if he did not love John Herschel so much, he should not hesitate to say he was the ablest and most worthy of the successors of Laplace. But there is no need to make the comparison, his works are with us.

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It is not for us to lift the veil which screens the sanctities of domestic life; but in the portrait we have sketched with a conscious feebleness, the reader will see what are the lineaments of a great Philosopher in his public relations; the mode, the aims, the hopes, the features, of his outer life. The noble example, the inevitable stimulativeness of such a life, are priceless. Though not lifting the veil of his home, we may say of him, that he there realised just what the reader would picture and would wish that life to be. He was a firm and a most active friend; he had no jealousies; he avoided all scientific feuds; he gladly accorded a helping hand to those who consulted him in scientific difficulties; he never discouraged, and still less disparaged, men younger than or inferior to himself; he was pleased with the appreciation of his works, but this was not an object of his solicitude; we quote the words of a discriminating critic, for they are not words of extravagance, when we say of him, that "his was a life full of the serenity of the sage, and the docile innocence of a child." Happy the pursuits that can lead to such results.

Herschel's whole life, like the lives of Newton and Faraday, confutes the assertion, and ought to remove the suspicion, that a profound study of Nature is unfavourable to a sincere acceptance of the Christian Faith.

Surrounded by an affectionate family, of which he had ever been the pride, the guide, and the life, John Herschel died as he had lived in the unostentatious exercise of a devout, yet simple, faith. The recollection of the long procession of his many friends, numbering among them some of the brightest and choicest intellects of his countrymen, still dwells vividly in our minds: what was mortal of him, we laid in Westminster Abbey, close by the side of Newton. The inscription on his monument is a happy but condensed expression, of the most deeply cherished thoughts of his life:—

JOANNES HERSCHEL
GULIELMI HERSCHEL
NATU OPERA FAMA
FILIUS UNICUS
"CŒLIS EXPLORATIS"
HIC PROPE NEWTONUM
REQUIESCIT
GENERATIO ET GENERATIO
MIRABILIA DEI NARRABUNT
PSALM CXLV. 4, 5.
VIXIT LXXIX. ANNOS
OBIIT UNDECIMO DIE MAII
A.D. MDCCCLXXI.

[C. P.]

Sir Roderick Impey Murchison, Bart. K.C.B., was born on February 19, 1792, at Tarradale, a small estate in the possession of his father, in eastern Ross-shire, N.B. He was the

eldest son of Mr. Kenneth Murchison, and of Barbara, daughter of the late Mr. Kenneth Mackenzie, of Fairburn, in the same county. At a very early age he left Scotland for Dorsetshire, and, excepting a short period when he resided with his mother in Edinburgh, his boyhood was principally spent in England, where he received his education. He was first placed in the grammar-school attached to the Cathedral of Durham, but exhibiting a strong inclination for the military profession he was removed to the Royal Military College at Great Marlow. He subsequently attended, for a few months only, the classes at the Edinburgh University, but his studies were interrupted by receiving, in 1807, a commission in the army, he at the time being just fifteen years of age. In 1808 he joined the forces in the Peninsula, under Sir Arthur Wellesley, first as ensign in the 36th Regiment of Foot, and afterwards serving either on the staff of his uncle, Sir Alexander Mackenzie, or as Captain in the 6th Dragoons. He shared the varied fortunes of the division of the army to which he was attached, having fought in three general actions, Roliça, Vimiera—where he carried the colours of his regiment—and Corunna, including all the hardships and dangers attending the memorable retreat under Sir John Moore. Returning to England, Captain Murchison married, in 1815, Charlotte, only daughter of the late General Francis Hugonin, and thus concluded, as he was frequently heard to remark, the military episode of his life.

Murchison was, however, of far too active a disposition to remain in a state of idleness at this time of life, so in the absence of better employment the exuberance of his spirits found vent in the excitement of the chase. Fox-hunting became one of his chief pastimes, and he entered into its pleasures with all the energy at his command. He might have continued a popular, sporting, country gentleman, but for the influence of Mrs. Murchison, a lady possessing a considerable knowledge of conchology and a fondness for natural history pursuits. Through her he received his first encouragements to pursue the study of geological science. It happened, also, that about this time he accidentally met Sir Humphry Davy at the house of Mr. Morritt of Rokeby. Sir Humphry, seeing too much practical sense in young Murchison for a mere fox-hunter, advised him to attend the lectures on physical science at the Royal Institution. He not only attended the lectures, but he followed them up by a series of practical experiments on his own account. To do this in a satisfactory manner he placed himself under the private instruction of the late Mr. Richard Phillips, F.R.S. From this date the scientific career of Murchison may be said to have commenced.

His first paper was presented to the Geological Society in 1825, entitled *A Geological sketch of the north-western extremity of Sussex, and the adjacent parts of Hampshire and Surrey.* It is published in Volume II. of the *Transactions* of that Society.

Murchison was now thirty-three years old, and henceforth devoted his whole energies to the pursuit of his adopted science. Year after year we find him occupied in the personal examination of the geological formations of various districts in Great Britain and on the Continent, beginning in 1826 with the coal strata in Sutherlandshire, which he demonstrated to be a branch of the Oolite series. In 1827 he again visited the Highlands in company with the now venerable Professor Adam Sedgwick, when they succeeded in proving that the primary sandstone of M'Culloch was really nothing more than the true old red sandstone, now also called "Devonian." In the following year, accompanied by Mr. (now Sir Charles) Lyell, he examined the volcanic rocks of Auvergne and the tertiary strata of Southern France. In this manner, first in one country and then in another, he continued his observations in the field, adding fact upon fact, all of which are recorded in the *Transactions* of the Geological Society and in other scientific publications. In the catalogue of scientific papers published by the Royal Society, Sir Roderick Murchison is credited with 111 separate papers, and twenty-six published in conjunction with other authors. To all the labour required in the preparation of such a mass of work must be added that necessarily involved in the writing of the valuable treatises, published separately — *The Silurian System* — *On the Geology of Russia* — and the successive editions of *Siluria*. The last-named is perhaps the best known of his writings, and, more than any other of his works, has helped to lay the foundation of Sir Roderick's fame. It is entitled *Siluria; or the History of the oldest known Rocks containing organic Remains, with a brief Sketch of the Distribution of Gold over the Earth;* and includes a general view of the structure of the Earth's crust, but more particularly of the more ancient series of strata, of which the Silurian system is the lowest; it also includes a summary of the author's opinions with respect to geological science, especially on those matters where he has differed from his distinguished friends, Professor Sedgwick and Sir Charles Lyell.

As a geographer, Sir Roderick Murchison stood in the foremost rank. The great influence he held over the proceedings of the Royal Geographical Society could be perceived in a moment by the most casual visitor to its ordinary meetings. No one took a greater interest in the progress of physical geography than he did, down to the last few days preceding his death. His annual addresses at the anniversary meetings of the Society always contained an excellent epitome of geographical discovery during the year. His kind and courteous manners, coupled with the zeal and energy with which, as President, he supported the cause and personal well-being of travellers, both before and after their journeys, made him at once their trusted counsellor and friend. His name is thus known to all the civilised world as the chivalrous defender of such men as Livingstone, Speke, Du Chaillu, and others, against the adverse criticisms of some of their fellow-geographers.

With the attention of Sir Roderick so engrossed in geological and geographical pursuits, he still found time to interest himself in some of the recent great discoveries of observational astronomy, especially of those relating to the physical constitution of the solar photosphere and corona. He, however, never took any active part in the promotion of astronomical research, for he wisely left to others, more qualified than himself, the practical part of our science. But in one important branch he has done good service. He took every opportunity of impressing upon travellers the advantages to be derived from the frequent determinations of the absolute longitudes and latitudes in unexplored regions by astronomical observations. He was therefore always anxious that travellers, before leaving England, should make themselves acquainted with the practical use of the sextant and other portable instruments. Through his recommendation, also, the accumulated observations of lunar distances, meridional altitudes, temperatures of the boiling point of water, &c., made by Captain Speke, M. Du Chaillu, and other African travellers, were placed under the care of a professional astronomer, in order that they might be reduced by the use of the most approved astronomical methods. In this manner the geographical positions of several important points in the interior of Africa have been satisfactorily determined. Sir Roderick also showed his interest in our Society, and in astronomy, by becoming, in 1850, one of our Fellows.

On the death of Sir Henry De la Beche, in 1855, Sir Roderick Murchison was chosen to succeed that eminent geologist as Director of the Museum of Practical Geology in Jermyn Street, and as Director-General of the Geological Survey of England. It is not, however, necessary in this brief notice to enumerate more than a selection of the honours, royal and academic, which have been conferred on Sir Roderick in recognition of his scientific labours. By his own sovereign he was created a Knight Bachelor in 1846, a K.C.B. in 1863, and finally, a Baronet in 1866. He also received numerous foreign orders of distinction, including the Grand Cross of St. Anne, and of St. Stanislaus of Russia, and the dignity of Grand Officer of the Order of the Crown of Italy. It is almost needless to add that the Universities of Oxford, Cambridge, and Dublin, recognised his researches in science by the bestowal on him of their honorary degrees. In 1826, he was chosen a Fellow of the Royal Society, and subsequently became a member of the Council and a Vice-President. He was one of the founders of the Royal Geographical Society, and was first elected its President in 1843. He was re-elected at intervals up to 1862, since which year, till May 1871, he, by the general consent of the Society, remained in office. He also served as Secretary and President of the Geological Society. In addition to the numerous English and Foreign Societies of which he was an honorary member, Sir Roderick was also a Trustee of the British Museum, the Hunterian Museum, and of the British Association for the Advancement of Science. He received the

Copley Medal from the Royal Society, the Prix Cuvier from the French Institute, the Brisbane Medal from the Royal Society of Edinburgh, the Wollaston Medal, and other honours of the same kind.

In 1869, Sir Roderick sustained a great loss by the death of Lady Murchison, his companion of more than half a century. The first visible sign of his own decay manifested itself in an attack of paralysis in the latter part of 1870. He was in a dangerous condition for some time, and he remained afterwards in a more or less delicate state of health. However, he was able occasionally to appear in public; but he was never thoroughly himself again, and was obliged to delegate to others the duty of Chairman at the meetings of the Royal Geographical Society. Two months before his death he was seized with loss of speech, accompanied with difficulty in swallowing. These unfavourable symptoms gradually subsided, and his general health appeared to be restored to its usual state. But on Thursday, October 19, he caught a cold during a drive, which subsequently resolved itself into bronchitis. His constitution was not sufficiently strong to withstand this sudden attack, and he quietly sank under its influence. He passed peacefully away on the evening of Sunday, October 22, 1871, in the eightieth year of his age.

"Such has been the praiseworthy life of this illustrious man," remarks a writer in the *Athenæum*; "and while his fellow-countrymen have long since pointed out with pride to the stranger the house where Murchison was born, there are mountains and rivers in distant lands which shall tell out his fame to future ages in tones far louder than could ever re-echo from the vaults of Westminster Abbey."

William Rutherford, LL.D. was for many years one of the mathematical masters at the Royal Military Academy, Woolwich, and one always popular with his pupils. He was a mathematician of no mean rank, and his name is well known as the author of several mathematical pamphlets; one of which, on the solution of spherical triangles, is particularly worthy of notice. Dr. Rutherford was also a regular correspondent, both as proposer and solver of problems, to the Mathematical Section of the *Lady and Gentleman's Diary*, and his successful solutions of some of the most difficult questions gained him considerable notice among the readers of that once popular annual. He delighted in what he used to call a "very pretty problem," that is, in some question involving a delicate mathematical analysis; and of late years, when he was what he called "an idle man," nothing pleased him more than to have submitted to him some question which, perhaps, might require a week's work before a solution was arrived at.

Dr. Rutherford had great skill as an instructor, his explanations being remarkable for their clearness. He also impressed on [illegible] of their treating the study of mathematics

as a means to an end, and not of looking on it as a subject by itself. Thus all his teaching was of a most practical character, and his mathematical examinations consisted of problems likely to occur to officers of the scientific branches of the army. He was of a quiet disposition, and was ever ready to give any aid to those who desired assistance from his knowledge and experience.

The Council of this Society had the advantage of the experience of Dr. Rutherford, as one of its members, during the interval between 1844 and 1857, and he served the office of Honorary Secretary in the two years 1845–6. He does not appear to have contributed to our *Transactions*, although he was well versed both in the theoretical and practical branches of astronomy. He was, however, always greatly interested in the investigations of some of his friends, and also in the principal subjects which occupied the attention of the Society during the time when he was an active member of the Council.

Dr. Rutherford retired from his office in the Royal Military Academy about eight years ago, after a very long service, and continued in the enjoyment of his usual good health until a few months preceding his death. He died at his residence, Maryon Road, Charlton, on the 16th of September, 1871, at the age of seventy-three.

The Rev. Thomas William Weare was the eldest son of the late Colonel Thomas Weare, K.H., Aide-de-camp to the Queen, and was born on November 2, 1813. He graduated at Christ Church, Oxford, in 1836. Subsequently to this he became Second Master of Westminster School, whence he retired in 1861. It was during the period of the tenure of this office that he was elected to the Fellowship of our Society, such election taking place on June 12, 1857. At this period, and up to the time of his quitting London in 1861, he was an occasional visitor at our Meetings, but ceased after this to attend them. His last appearance among us was on February 11, 1870, on the occasion of the fiftieth anniversary of the Society. He was appointed Rector of Isfield, in Sussex, in 1867; but mainly resided at his seat, Hampton House, in Herefordshire, for which county he was a Magistrate. He died at Speen, in Berkshire, on February 24, 1871.

Proceedings of Various Observatories.

Royal Observatory, Greenwich.

The only change in the usual course of observations during the past year has been the substitution of the Water-Telescope for the Reflex Zenith-tube in the spring and autumn observations of *γ Draconis*.

The state of the reductions is highly satisfactory, the greater

portion of the MS. for 1871 being already in the printer's hands, while the printing is in a more advanced stage than usual, as the whole of the Volume for 1870 has been printed off.

A well-marked discordance between the zenith-points deduced from the Nadir-observation, and those from stars observed by reflection with the Transit-Circle, has led to the application of a correction of $-0''\cdot82$ to the former. This naturally threw suspicion on the corrections for errors of graduation, in consequence of which a new determination of these errors for every 5° has been made, but no change in the circle-readings can be traced with any degree of certainty. It may be remarked that the discordance alluded to changed its sign with the flexure and R—D correction when the cube was pierced, being then hardly sensible; it has since gradually increased to its present value, at which it has now remained constant for more than a year.

The flexure of the Transit-Circle has been re-determined, and appears to be smaller by $0''\cdot25$ than the value previously adopted; a re-examination of the pivots agrees with former measures in the satisfactory conclusion that there is no sensible error of form.

Great attention has been given to the phenomena of *Jupiter's* satellites, more especially those of the third and fourth, which have been observed on almost every occasion when the weather permitted, though in many cases interfering seriously with the meridional observations. The results have been laid before the Society.

A close scrutiny of *Venus* with the great Equatoreal under favourable atmospheric conditions has failed to establish the existence of any decided markings on her disk or notches in her limb.

For a short period Encke's Comet was bright enough to be observed with the Transit-Circle, and its place was thus determined on several nights. The appearance of this object in the great Equatoreal is shown in a drawing made by Mr. Carpenter and exhibited to the Society. A woodcut taken from this drawing will be found in the *Monthly Notices*.

The uncertainty in the determination of fundamental R.A. arising from irregularity of clock-rate has been reduced to an inappreciable quantity by the substitution of a new and improved clock for the Transit-Clock Hardy. The new clock is placed in the Magnetic Basement, which, for other purposes, is maintained at a temperature as nearly uniform as possible, and this, no doubt, is an important element in the success of the clock. The chief peculiarities of construction of this Normal Sidereal Clock (the work of Messrs. Dent and Co.) are in the escapement and in the pendulum; the former is similar to the detached escapement used for chronometers and which has been already applied successfully to clocks at Greenwich; the latter, of the same form as that adopted for the Westminster clock, may be briefly described [illegible] follows:—A steel rod supports a zinc tube from which an [illegible] tube is suspended, carrying at its lower extremi t

a leaden bob attached at its centre of gravity. Thus the expansion of the zinc tube compensates those of the steel rod and tube, the coefficient for zinc being, as is well known, nearly three times that for steel. This construction has also been applied with great success to the principal clocks intended for the Transit of *Venus* expeditions. In the Normal Sidereal clock there is also a very delicate means of making the final adjustment for compensation, but no necessity has yet arisen for trying this. The very slight changes of rate observed in the clock appear to take place very gradually and to have no connexion with temperature. As yet there are hardly sufficient data to establish their dependence on atmospheric pressure; but it seems that every considerable rise or fall of the barometer is accompanied by a corresponding increase or decrease of the losing rate.

Radcliffe Observatory, Oxford.

No change worth mentioning has taken place in the personal establishment or in the operations of the Radcliffe Observatory during the past year. The amount of work which has been performed both in the astronomical and the meteorological departments is nearly the same as that of the preceding year, and the reductions are kept up to almost the same state of forwardness, though, with every possible exertion, it is found impossible to gain much advance, either in the reductions or the printing. The greater portion of the astronomical part of the volume for 1869 is printed; and it is hoped that a few copies of the Catalogue of Stars for that year, amounting in number to nearly 1500, will be ready in a very short time. During that year, and those which have followed it, great attention has been paid to daylight observations of stars, and it may be mentioned that the rate of the transit-clock (since a little more mercury was put into the cylinders in 1868) has been exceedingly uniform. There is no difference throughout the year which can be attributed to temperature; and the slight deviations from uniformity, which are generally not of the nature of bad going or casual irregularity, afford scope for some interesting speculations.

It may be considered now that the materials for the Third Radcliffe catalogue are complete, and Mr. Main purposes to take in hand the compilation of it as soon as it is possible, with due regard to the progress of ordinary work, combining the observations of all stars from 1862 to 1871, both years inclusive. The weather during a great portion of the year has not been favourable to observations with the Heliometer, though a considerable number of double stars have been observed in the short intervals of fine weather. It may finally be mentioned that, commencing with October last, thirteen occultations of stars by the Moon have been observed, the greater number being at very low

altitudes, and therefore being available for the correction of the Moon's parallax.

Cambridge Observatory.

The new Transit Circle has been chiefly employed in determining the places of small stars in the zone lying between 25° and 30° of North Declination, a work which has been undertaken by this Observatory with the view of assisting in carrying out the plan formed by the German Astronomical Society for observing all the stars, to the ninth magnitude inclusive, contained in Argelander's *Durchmusterung des nördlichen Himmels*.

Upwards of 4,500 of these stars have been observed, and their true North Polar Distances accurately calculated.

All the instrumental and clock corrections have been obtained, and are ready to be applied for finding the Right Ascensions, as soon as the reductions to the centre wire, now in progress, are completed. These observations were taken with the instrument clamped in a fixed position, the bisections being made by means of the micrometer, and each star was observed at three of the vertical wires. In this way the stars within a range of about 10′ in declination could be conveniently and rapidly observed. The circle readings were taken at the beginning and end of each set of observations, and were found to remain remarkably constant. The optical power of the instrument is so great, that stars down to the tenth magnitude can be observed, and consequently, in the mode of observation just described, many more stars were taken than were required in order to carry out the plan laid down by the German Astronomical Society. Accordingly, the plan of observation has been lately modified, so as to make it conform more closely to the programme of the Society, and Mr. Simms has made a few alterations in the instrument which have rendered it more suitable for the class of observations required.

The bisection of a star with a single declination wire was found to interfere seriously with the right-ascension observations, so that in the case of very small stars it was found necessary to complete the right-ascension observations before the bisection was made. In order to remedy this, the single wire has been replaced by two parallel wires, 10″ apart; and two similar pairs of wires have been added, each at the distance of about 5′ from the central pair, to give increased facility of bisection in particular cases.

The tangent screw handles have been made more accessible to the observers, one being under the control of the assistant who reads the microscopes, and enabling him to set the instrument with great rapidity within a range of more than half a degree; the other is controlled by the right-ascension observer who makes the bisection.

Mr. Graham has adopted a method of determining the intervals

of the right-ascension wires which has been found to be very accurate and convenient. The eyepiece is turned through 90°, and each of the right-ascension wires, now become horizontal, is in turn directed to the horizontal wires of either collimator, and the corresponding readings of both circles are taken.

The values of the revolutions of the micrometer-screws have been found in a similar way with great accuracy.

Small buildings have been erected over the Collimators, which are now therefore virtually in the same room with the Transit Circle, and the consequent steadiness in the images of the collimator wires is very remarkable.

For clock corrections the catalogue of 529 stars issued by the German Astronomical Society has been used. About six of these stars are generally taken in the course of the night's work in addition to the stars near the pole which are observed for instrumental deviation. For instrumental errors, *Polaris* has been observed sixty-six times above the pole and seventy-six times below; and δ *Ursæ Minoris* eleven times above and ten below; 51 *Cephei* seven times above and two below; and λ *Ursæ Minoris* five times above and three below. *Neptune* has been observed eleven times near the opposition.

Observations for finding the errors of level and collimation, and for determining the zenith point, are made daily in observing weather. The error of collimation has been determined 189 times, the error of level 227 times, and the zenith point 236 times. All those errors have been calculated and applied to the observed transits of standard stars throughout the year. The clock corrections and rates are also calculated.

Mr. Simms has rendered the counterpoises much more sensitive by supporting the fulcrums of the levers on knife edges. He has also made some improvements in the driving-clock of the Northumberland Equatoreal, though its performance is still far from satisfactory.

Very little time could be given to extra-meridional observations, but some good observations were obtained of Encke's Comet. The meteorological observations have been made at the usual hours of nine A.M. and three P.M.

Dunsink (Dublin) Observatory.

During the past year Dr. Brünnow has completed the observations on the parallax of five stars, namely, *Groombridge* 1830, *Bradley* 3077, 85 *Pegasi*, 6 *Draconis*, and α *Lyræ*, the two latter having been compared with different stars from those used in the former series. These observations have all been computed and their results are now being printed. Two new series have been commenced on the parallax of the Planetary Nebula, H. IV. 37, and on 61 *Cygni*. The meridional observations have been interrupted for the greater part of the year, as the Transit

instrument was taken down, and the Circle admitted only of a very limited use, on account of extensive alterations in the observing-room, which were required for the mounting of a large Meridian-circle, which is shortly expected.

Royal Observatory, Edinburgh.

A meeting of the Board of Visitors of this Observatory was held in July, and a Report, then presented to them by the Astronomer, was afterwards printed and published in November. It contains twenty-six pages of letter-press with five plates, and sufficiently describes all details of work up to that date.

Since then, on January 17, the Astronomer writes that he is within a few pages of the end of Volume XIII. of the Edinburgh Observatory series. It is the largest volume yet issued from this establishment; it has occupied upwards of two years uninterruptedly in preparation, and contains all the more useful observations made during the interval from 1860 to 1869, with some additions to 1871.

Durham Observatory.

After holding the Directorship of the Durham Observatory for more than thirty years, the Rev. Professor Chevallier has resigned that trust during the course of the past year, to the deep regret of all interested in the progress of practical astronomy and the welfare of this establishment. No successor to Mr. Chevallier has yet been appointed.

The work performed during the past year has been chiefly the extra-meridional observation of minor planets, as in previous years, preferring those more recently discovered, which have been bright enough to come within the optical means of the Equatoreal. Some trustworthy observations of Encke's Comet have also been obtained. The reductions of these are all in an advanced state, and will be communicated shortly to the *Astronomische Nachrichten*. Two abstracts of results of Equatoreal work have been published in that journal during the past year, the first communication being of Cometary, the second of Planetary Observations. The spectrum of the Aurora Borealis has been examined on several occasions, notably on November 9 but, beyond the verification of the well-known green line, with only negative results. Some success attended an attempt to measure the diameter of the first optical ring of light round the image of the star *Polaris* by its transit over the meridional wires of the Transit Circle; but the observation, though easily and satisfactorily made, and though agreeing with a similar determination made two years ago, does not agree with the diameter which theory assigns for the object-glass employed.

The regular course of meteorological observations has been continued without intermission, forming the twenty-second year of the series, and an annual meteorological summary has been compiled as usual; but it remains only in manuscript.

Glasgow Observatory.

The operations at the Glasgow Observatory during the past year have not been distinguished by any deviation from the system pursued in former years. The superintendence of the electrical arrangements connected with the transmission of Greenwich mean time to the City and Port of Glasgow has recently been transferred from the Magnetic Telegraph Company to the authorities of her Majesty's Post Office, and is characterised by the same efficiency as heretofore. Prof. Grant hopes in the course of another year to bring to a close a series of star observations with the Transit Circle which for several years past have occupied the whole resources of the Observatory, and that henceforward he will be enabled to devote more attention to observations out of the meridian. A few observations of the Comets of Encke and Tempel have been obtained with the Ochtertyre equatoreal.

Liverpool Observatory.

Bidston, Birkenhead.

The work at this Observatory during the past year has been chiefly confined to the communication of time to the Port, the testing of nautical instruments, and to meteorological observations.

The firing of a gun at 1^h P.M. daily is more highly appreciated than any other method of communicating time that has been adopted at Liverpool; the flash of the gun is well seen from the river and docks, and the sound is well heard in the towns of Liverpool and Birkenhead.

During the past year between three and four hundred chronometers have been tested. There appears to be wanting for the Mercantile Marine some standard of authority to appeal to for those who are interested in the performance of chronometers, in order to judge if their own instruments are of average goodness as regards regularity of performance and thermal adjustment. It is hoped that the method of exhibiting the rates of these instruments now in use at the Liverpool Observatory may to some extent be found to supply this deficiency.

The certificates of test supplied to the owners of the instruments which are sent to this Observatory show the daily rates for five consecutive weeks.

The temperature is changed $15°$ at the end of each week throughout the year, and all the chronometers at the Observatory

are subjected to the definite temperatures of 55°, 70°, and 85° of Fahrenheit in succession.

In Mr. Hartnup's report to the Mersey Docks and Harbour Board for the year 1870, it will be seen that, for 381 chronometers tested in this way, the mean of all the greatest differences of daily rate between any two weeks is 2^s·14 and that 1^s·61, or about three-fourths, of this appears to be due to error in thermal adjustment.

In order to render the information thus obtained available for comparison the rotation number, showing the order in which the instrument was received at the Observatory, has been supplied with each certificate of test. At the end of each year an abstract of the rate of each chronometer with its rotation number is published so that by preserving this number the owner of any chronometer tested at the Observatory may, by means of the printed abstracts, compare its performance with that of any other instrument, or with the average of all tested during the year, and may immediately see whether the regularity of performance was better or worse than the average.

The Liverpool Observatory was not originally supplied with self-registering meteorological instruments, but their erection was subsequently urged upon the authorities of Liverpool by merchants and scientific gentlemen resident in the town and neighbourhood. Applications are frequently made for extracts from meteorological records months and even years subsequent to the date on which the various phenomena have been recorded, and a table of results for the year 1870, which is intended to supply the information generally applied for, has been published in the Report for that year.

The testing of nautical instruments, and the taking and reducing of meteorological observations, have so much occupied the time of Mr. Hartnup during the past two years, that with but one assistant he has been able to do but very little work with the equatoreal.

Stonyhurst Observatory.

The chief improvement since last year is the adaptation of [illegible] photographic apparatus to the Equatoreal, with the view [illegible] [illegible] pictures of sun-spots. Images of various si[illegible] [illegible] during the summer months, and the sc[illegible] [illegible] was that of 17½ inches to the solar diameter. [illegible] the photographer of the Preston Town Council, [illegible] in making the necessary arrangements. The [illegible] procure a series of photographic records of the [illegible] rapid, changes in any remarkable spot, or [illegible]

[illegible] magnetic department of this observatory [illegible] undertaken during the months of [illegible] The magnetic declination, dip, and

horizontal force, were observed at about 20 stations, and these observations are at present in course of reduction.

Kew Observatory.

The photographic solar observations have proceeded during the last year with great regularity, under the superintendence of Mr. De La Rue, and have been so much favoured by the weather, that on 226 days 381 photograms have been taken, a result far exceeding that of any previous year. The reductions and investigations necessary for the intended determination of the Sun's elements have progressed vigorously, and are now completed up to the end of the year 1869, while two independent papers, one having reference to a distinct law in the form of the sun-spot curve, and published by the Royal Society; the other, giving further evidence of the interplanetary relations of the Sun as shown by its activity, and handed in for publication,—have also been prepared during that time. The special summary for last year will be laid before the Royal Astronomical Society very shortly.

The past year is the last in the ten-yearly period during which the Photoheliograph at Kew has been at work, and within a very few days the continuous photographic record of the Sun's disk will in this country draw to a close. How great a loss this is for astronomical science, especially at the present time when the inquiries into the physical nature of the Sun form so prominent a part in the activity of astronomers, will be seen by the statement that during these ten years no less than 2778 solar photograms have been taken, preserving almost for all time the most faithful record of fleeting phenomena, still surrounded with profound mystery and great interest, and still promising the most surprising disclosures of the widest bearing. About twenty papers, embodying laborious investigations and discussions, have been communicated during that time to the Royal Society and the Royal Astronomical Society, by the observers engaged in the Kew solar researches, and have been published.

The completion of the reductions for the years 1870 and 1871, as well as the final discussion of the whole series, will be completed at Mr. De La Rue's expense, and will take about one year and a half. The paper containing this last instalment of the work will, it is confidently expected, be a conclusive proof in itself that solar-eye observations can no longer compete with photographic in the absolute reliability of the facts deduced. Concurrently with the discussion of the Sun-spots, the relative amount of faculæ will be measured in the Kew pictures, as there is reason to think that important conclusions may be drawn in regard of solar activity by a study of the faculæ. All the Kew pictures may not be available for this purpose, but in future, pictures can be taken in which the faculæ will be most carefully defined, by so regulating the exposure of the photograph that these entities may be rendered very

conspicuous. It is satisfactory that there is a prospect that a record of the Sun's activity will be continued by the Wilna Photoheliograph; and that, after the lapse of a year or two, a new Photoheliograph, with all those improvements and perfections which advanced knowledge and past shortcomings have suggested, will be erected in this country by Mr. De La Rue. In the meantime the Royal Society have sanctioned the replacement of the secondary magnifier (an ordinary Huyghenian eye-piece) of the Kew instrument by a positive eye-piece achromatised for the chemical rays, as in the Wilna instrument.

Statement of the work done at the Kew Observatory with the Heliograph.

Year.	Number of Days of Observations.	Number of Pictures Obtained.	Remarks.
1862	163	227	Pictures taken at Cranford.
1863	125*	184	Instrument mounted at Kew. Pictures commenced in May.
1864	164	249	
1865	159	277	
1866	157	262	
1867	131	187	No observations made between Aug. 9 and Sept. 9; building under alteration.
1868	174	285	
1869	195	324	
1870	220	381	
1871	226	381	
1872	10	21	January only.
Total	1724†	2778	

Mr. Huggins' Observatory.

During the past year the large Grubb Equatoreal, and the new spectroscopes constructed for use with it, have been got into adjustment, and some work has been done, though the long continuance of bad weather during the autumn left but few favourable nights for observation.

Measures have been taken of the remarkable bands of absorption in the spectra of *Uranus* and *Neptune*. The spectra of Comet I. 1871 and Encke's Comet were found to be similar to those of former comets examined at Tulse Hill. An account of these observations and drawings of the appearance of Encke's Comet will be found in the *Proceedings of the Royal Society*.

* Spots were unusually scarce this year.

† The number of good observation days seem to be greatest at the times of maximum spots.

Some observations on the spectra of nebulæ, and on the motion of stars in the line of sight, are at present not completed; but it is expected that the velocity of *Sirius* from the Earth will be found to be smaller than the estimate which Mr. Huggins obtained with the less powerful instruments then at his command.

Some quite recent observations of the change of refrangibility of the Fraunhofer lines at the opposite limbs of the Sun appear to show a motion in the atmosphere producing the Fraunhofer lines greater than that which is indicated for the photosphere by the movements of the spots.

Mr. Bishop's Observatory.

The principal work of the past year has been the continuation of the charts or stars within three degrees north and south of the ecliptic, to the eleventh magnitude inclusive. It has been found necessary to introduce a considerable increase in the measure of the square degree for those parts of the ecliptic where it is traversed by the Galaxy, in order to show distinctly the great number of small stars which the charts include. Much additional zone-work has also been required, but the whole is now in a sufficiently advanced state to allow of the re-issue of the charts commencing in the present month, with the hope of maintaining a regular publication during the year. In this district, the nights available for small-star-work have been by no means up to the average. Encke's comet has been satisfactorily observed for position on sixteen nights up to December 6. The small comet, discovered by M. Tempel, on November 3, was observed on four nights. It is Mr. Bishop's intention to combine calculation, with observation, in the proceedings of his Observatory. During the past year a full discussion of the observations of the Great Comet of 1861 has been completed by Mr. W. E. Plummer: the number of observations approaches one thousand; all sensible planetary perturbations have been taken into account, and the final orbit depends upon normals on which the whole of the observations about the respective dates have been brought to bear. The main result of the investigation is to indicate that the comet was moving in an ellipse with a period of 402 years; the details are now in course of arrangement for publication. Upon the suggestion of Mr. Hind, Mr. Bishop has undertaken the calculation of the effect of planetary attraction upon Encke's comet during the next revolution, and hence the preparation of an ephemeris for the reappearance in the spring of 1875. Very little has been done in England in this department of astronomy, and although it has been announced by the German Astronomical Society that an experienced computer proposes to occupy himself with a similar investigation, it appears to be intended to apply a new method, and it will be at least satisfactory in this case to possess a duplicate calculation. Mr. Plummer is now engaged in the preliminary correction of the predicted elements from the observations of 1871, after which he will apply

with every accuracy the method of computation indicated by the Astronomer Royal, with certain modifications to suit this particular case. An ephemeris of Tuttle's comet, of short period, for the Southern hemisphere. was issued from this observatory, by the aid of which Mr. Stone had been able to observe it at the Cape of Good Hope in the middle of December. The periodical comet of De Vico, which has not been detected since its appearance in 1844, has been sought for on the assumption that a perihelion passage might take place in the autumn of 1871. It now appears probable that this did not occur between August and November.

Royal Observatory, Cape of Good Hope.

The chief attention of the staff has been directed to the reduction and passing through the press of the observations made with the Transit-Circle, in order to bring forward a catalogue of stars from observations made since the erection of that instrument. The Transit-Circle was brought systematically into use in 1856. The observations made in that year have been reduced, printed, and distributed. The observations made in 1857 have been printed, Those in 1858 have been partially printed. The reductions of the observations made in 1859 are in a forward state. The meteorological observations from 1841 to 1870 have been collected, discussed, and the results printed and distributed. The miscellaneous magnetical observations, not hitherto printed, have been collected, reduced when necessary, the means taken, and the results prepared for press. It is intended to append these miscellaneous observations to the volume containing the results for 1857 and 1858, which it is hoped may soon be ready for distribution.

The work of the Observatory has been much impeded since August by the illness, at one time, of no less than three of the staff, and the lamented death, on September 18, of Mr. Sinfield, an assistant of considerable experience and much promise. Much time has been devoted to the examination of the form of the pivots and the division-errors of the Transit-Circle. The observing with the Transit-Circle has been chiefly directed to observations of the zenith distance of circumpolar stars, wide and close, both above and below pole, to afford material for a separation of any existing errors in the refractions adopted, and the colatitude of the observatory. Observations of α *Centauri*, β *Centauri*, and α *Eridani*, have been made to test the truth of the reference of a large systematic annual change in the zenith distances of α *Centauri*, observed at this observatory, to annual parallax.

A complete reobservation of all stars down to the seventh magnitude within 5° of the South Pole has also been made. Each star has been observed at least three times.

The Equatoreal has been employed upon observations of Winnecke's Comet I. 1871, Tuttle's comet, and a reobservation of many of Herschel's double stars. A few attempts have been

made to use the spectroscope with it, but without much success. The mounting is too weak and the driving-clock too defective for any employment of the Equatoreal upon such work with advantage. The aperture of the object-glass, seven inches, is small for such work upon faint objects.

The November meteors were carefully looked for, but very few were seen.

It is intended this year to observe the principal stars within the ecliptic limits for absolute right ascension—a point which has not previously received attention here. The changes of temperature between day and night are very considerable, and it has been thought necessary to protect the chief sidereal clock as far as possible against exposure to these changes. A change has, for this purpose, been prepared in the interior of the building where the changes of temperature from day to night are confined within a degree. The clock will stand upon a pier raised free from the walls and floors directly from the rocks below the building.

It is intended to repeat during the winter season the observation of the stars within 5° of the South Pole. Such observations are thought likely to be of value both for the determination of azimuthal errors in the Southern hemisphere, and also at some future period for an accurate determination of the constants connected with the motion of the polar axis by reference of its position to the mean of the group of stars rather than to one or two selected stars.

Sydney Observatory.

Mr. Russell, who succeeded Mr. Smalley in the charge of this Observatory, read a report on the state and progress of the Observatory during the year 1870, at the Annual Visitation held on September 7, 1871. A few extracts from the Report are here given :—

"From the commencement of the year up to the time of his lamented death on the 12th of July, 1870, the late Astronomer had charge of the base-line operations at Lake George, in addition to the Observatory, and during that time all that his failing health allowed him to do was devoted to the base-line. Greater part of the Computer's time was also given to the examination of the instruments, measuring-bars, and other things connected with the same work. Little, therefore, could be done in the Observatory except the regular observations with the transit-instrument, the meteorological work and its regular reduction and publication, together with the examination and preparation of thirty sets of meteorological instruments for the new stations.

"The transit-instrument was in constant use, but not in good order; the dust, which got to the bearings in spite of close-fitting caps, cut into the soft pivots, and then they were made rough into the agate planes and removed the polish, so that the wear was

very considerable (this has since been remedied and the instrument put in order). . . . In spite of every precaution most unsatisfactory jumps occur in the corrections of the transit, which can only be attributed to the quarrying, and especially the blasting, operations carried on near the Observatory. One person is removing stone from his own land, and has several times shaken the Observatory in such a way that no instruments could be expected to be accurate.

"I would strongly urge the advisability of getting a new transit-instrument, which shall be equal to the exact requirements of modern astronomy. With an instrument at the Cape of Good Hope equal to the Greenwich transit, and one almost equal at Melbourne, it seems a waste of energy to work with an instrument that was made more than thirty years since, and which never was a good one, even when new, either in plan or construction."

During the year 1870, 952 transits were observed, in addition to the usual observations for the instrumental adjustments, and 270 measures of the distance and angular position of double stars. Among the subjects of observation carried on in 1871, special attention was given to meridional observations of the Moon and Moon-culminating stars for the determination of the longitude, and to the measurement of distance and angle of position of as many circumpolar double stars as possible, and to the observation of the spectrum of a few of them.

Notes on some Points connected with the Progress of Astronomy during the Past Year.

The progress of Astronomy has been satisfactory during the last twelve months, as may be gathered from the various important subjects briefly alluded to in the following notes. The principal observatories have been steadily recording the results of their arduous and useful labours, storing up data for the future benefit of our science; while other observers, with less routine duties, have found time and zeal to discover several minor planets and comets, in addition to the usual amount of miscellaneous work. But perhaps the most important Astronomical observations during the year, and to which most attention has been given, were those of the Total Solar Eclipse of December 11 (Dec. 12, Civil reckoning), visible in Southern India, Ceylon, and Northern Australia. The generally successful results obtained in India and Ceylon, both with the spectroscope and by photography, will probably explain definitively many undecided points relating to the constitution of the Sun's chromosphere and corona.

Total Solar Eclipse of Dec. 11 *(Dec.* 12, *Civil Reckoning),* 1871.

In the last Annual Report will be found a summary of the observations of the Total Eclipse of December 1870. The bad

weather which prevailed at many of the observing stations rendered the results of this eclipse far less complete than was expected from the careful preparations which had been made. The new information, though a considerable and important gain, was not complete enough to warrant a definitive opinion on the several points connected with solar phenomena on which we were seeking to increase our knowledge. The extent and nature of the coronal light and rays surrounding the eclipsed Sun were left so far not conclusively settled as to allow great differences of opinion still to exist.

It was therefore most fortunate for science that another Total Eclipse visible in our own possessions,—in India and in the north of Australia,—would take place during the past year.

We rejoice to learn that the weather was favourable at nearly all the observing stations in India, and that very satisfactory observations and good photographs have been obtained. It is with great regret that we receive a telegram to say that bad weather prevailed in Australia. The reports which have reached the Council are as yet too incomplete for it to be desirable to attempt any discussion of the bearing of the observations on the great questions of the true nature and extent of the coronal light and rays. Such a discussion can be only properly made when the reports of all the observers have been received, and when the valuable photographs of the Sun's surroundings obtained at the different stations have been carefully examined and compared with each other. We therefore confine ourselves to giving some of the principal results obtained at each station as far as they have reached us, in the observers' own words.

Colonel Tennant, who through the liberality of Lord Mayo had been able to organise a strong observing party, including Capt. Herschel and Mr. Hennessy, selected the old meteorological station of Dodabetta, on the highest peak of the Neilgherries, 8650 feet above the sea. He telegraphed his principal results in the following words, "Thin mist, spectroscope satisfactory. Reversion of lines entirely confirmed. Six good photographs." In a letter addressed to one of the Secretaries, Col. Tennant writes:—

"I had my attention specially directed to the chromosphere and prominences at the true bottom of the Moon's limb. They first appeared *white*, and then changed through pink to red. I am quite confident that I saw no blue or green tinge. You will remember my suspicion that I do not readily see blue when faint, but I am sure the tint was not strong. My waiting in the Moon's centre gave me an opportunity of looking about me, and I saw the two rifts I have spoken of. To me they did not reach the Moon's edge, but were separated by some minutes of bright corona. The corona's outer layers were undoubtedly radiated, but not coloured. There were alternate gradations of light; that is, light and comparative darkness, but certainly no colour. The rays were lost as they neared the Moon. The rift at the

true vertex of the Sun I am sure did not change, nor I believe did any other. Colonel Saxton had made for my object-glass a loose cap of pasteboard, with a two and a half inch aperture to be used during the Sun's presence; this was to have been knocked off just before totality. I forgot this, but I doubt if I should have improved matters; I used the power 35.

"I had asked Captain Morant, R.E. (to whom I am indebted for a very great amount of assistance), to take a very beautiful reconnoitring telescope by Dallmeyer, which was mounted on a stand. Of 14 inches focus, it has a power of 15 and an aperture of 1·75 inches. He (Captain Morant) made two sketches of the corona, which generally confirm, as does his description, all I have described. He expressed himself positive as to the absence of colour and the permanence of rifts, &c., in certain positions (not mine), and only differed in thinking that the shades were of a sepia tinge (brownish) and that the rifts did extend to the Moon."

The Madras astronomer, Mr. Pogson, observed at Avenashy. He telegraphed to the Astronomer Royal, "Weather fine, telescopic and camera photographs successful, ditto polarisation; good sketches; many bright lines in spectrum."

In a letter to the Astronomer Royal, Mr. Pogson writes:— "As my telegram informed you, we were upon the whole successful—more so than I ventured to anticipate—much less so than I could have wished. With the spectroscope I saw five lines; one bright and fine, right across the field, evidently my old Masulipatam friend, which the Americans call 1474. This was, I believe, the only bright corona line—the *only one I saw*—the other four were only part-way across, two in the blue and two near together in the deep red, *i. e.* where such colours are assuredly seen, and certainly prominence lines. With the Browning reflector my son and Col. Ritherdon together got three photographs, all available, but two, as I consider, *very* good. These show the corona some five or seven minutes in extent, but are all over-exposed. Thirty seconds each was allowed, as suggested by Mr. De La Rue; but I think ten would have been better, and we could have had more views. With a camera four views were taken—three very good—by Mr. Cruths, a professional. The polarisation was, I believe, very good and satisfactory; measured near the Sun's south (right hand) limb, and some distance off, and decidedly stronger further off the Sun than close to it. The sketches were very good, and will, I think, be valuable and reliable, and the ordinary contacts and some distances and angles of position of the cusps were also carefully secured."

Dr. Janssen has addressed the following letter to the President:—

J'aurai l'honneur d'adresser à la Société Royale Astronomique un mémoire détaillé de mes observations de l'Eclipse, mai je profite du départ de ce courrier pour vous informer des principaux résultats obtenus.

Sans entrer dans une discussion qui fera partie de ma relation, je dirai d'abord que la magnifique couronne observée à Sholoor s'est montrée sous un aspect tel qu'il me paraîtrait impossible d'admettre ici une cause des phénomènes, ou de diffraction, ou de réflexion sur le globe lunaire, ou encore de simple illumination de l'atmosphère terrestre.

Mais les raisons qui militent en faveur d'une cause objective et circumsolaire prennent une force invincible quand on interroge les élémens lumineux du phénomène.

En effet, le spectre de la couronne s'est montré dans mon télescope, non pas continu comme on l'avait trouvé jusqu'ici, mais remarquablement complexe. J'y ai constaté :

Les raies brillantes, quoique bien plus faibles, du gaz hydrogène qui forme le principal élément des protubérances et de la Chromosphère ;

La raie brillante verte qui a déjà été signalée en 1869 et 1870, et quelques autres plus faibles ;

Des raies obscures du spectre solaire ordinaire, notamment celle du sodium (D.) Ces raies sont bien plus difficiles à apercevoir.

Ces faits prouvent l'existence de matière dans le voisinage du Soleil, matière qui se manifeste dans les éclipses totales par des phénomènes d'émission, d'absorption, et de polarisation.

Mais la discussion des faits nous conduit plus loin encore.

Outre la matière cosmique, indépendante du Soleil, qui doit exister dans le voisinage de cet astre, les observations démontrent l'existence d'une atmosphère étendue, excessivement rare, à base d'hydrogène, s'étendant beaucoup au de-là de la Chromosphère et des protubérances, et s'alimentant de la matière même de celles-ci, matière lancée avec tant de violence à travers la photosphère, ainsi que nous le constatons tous les jours.

La rareté de cette atmosphère, à une certaine distance de la Chromosphère, doit être excessive ; son existence n'est donc point en désaccord avec les observations de quelques passages de comètes près du Soleil.

Neelgherry, Sholoor, 19 *Décembre,* 1871.

From the English party sent out at the expense of the Government at the instance of the British Association, the following reports have reached us.

Mr. Lockyer, who observed at Bekul, speaking of the corona says, "Its rays arranged almost symmetrically, three above and three below two dark spaces or rifts at the extremities of a horizontal diameter. The rays were built up of innumerable bright lines of different lengths, with more or less dark spaces between; near the Sun this structure was lost in the brightness of the central ring. I next tried the spectrum of a streamer above the point at which the Sun had disappeared. I got a vivid hydrogen spectrum with 1474 (I assume the point of this line from observation) slightly extended beyond it, but very faint throughout its

length compared with what I had anticipated and thickening downwards like F. I was, however, astonished at the vividness of the C line and of the continuous spectrum, for there was no prominence on the slit; the spectrum was undoubtedly the spectrum of glowing gas. In the Savart I saw lines vertical over everything, corona, prominences, dark Moon, and unoccupied sky." With a simple train of prisms Mr. Lockyer saw "four exquisite rings with projections where the prominences were. In brightness C came first, then F, then G, and last of all 1474. The rings were nearly all the same thickness, certainly not more than 2′ high, and they were all enveloped in a line of impure continuous spectrum." "The structure of the corona was simply exquisite and strongly developed, I at once exclaimed, '*Like Orion*.' Thousands of interlacing filaments varying in intensity were visible. I saw an extension of the prominence structure in cooler material. This died out somewhat suddenly some 5′ or 6′ from the Sun, and then there was nothing. The great fact was this, that close to the Sun and even for 5′ or 6′ away from the Sun, there was nothing like a ray or any trace of radial structure." At this station five good photographs were obtained.

Commander Maclear observing at the same station says, "As totality came on the light decreased and the lines increased exceedingly, rapidly in number and brightness, until it seemed as if every line in the solar spectrum was reversed; then they vanished not instantly, but so quickly, that I could not make out the order of their going, except that the Hyd. D *b*, and some others between D and *b*, remained last. Then they vanished and all was darkness. I then unclamped and swept out right and left, but saw nothing; then went to the direct vision, but saw nothing; placed the telescope on the Moon's limb by the eye-piece, then put in the spectroscope, but the light was not sufficient to show any spectrum; pointed the telescope carefully first on the dark Moon, and then on a bright part of the corona, but no spectrum. I then looked at the corona with the naked eye, saw a bright glory round the Moon, stellar form, six-pointed something like the nimbus painted round a saint's head, extending to a diameter and a half. Looked through the finder and saw the same form, but very much reduced in size and brilliancy, then examined with the 6-inch and eye-piece and saw nothing but a bright glow round the Moon, not much more than the height of the big prominence plainly visible in the south-east quarter."

Prof. Respighi observed at Poodocottah. He writes, "Towards the end of 1868 a small flint-glass prism was made for me by Signor Merz of Monaco to be fitted to the object-glass of the equatoreal of the observatory at Campodoglio. This apparatus, in consequence of the dispersion of the prism and the goodness of the prism and object-glass, was found to be admirably adapted for observing the eclipse in the manner just described (that is, without a slit, so that the several chromatic images of the corona would be simultaneously seen in the same field of view).

"The dispersion of the prism from lines C to H is about 32′; the free aperture of the object-glass is $4\frac{1}{3}$ French inches; the field of the telescope about 1°, with a magnifying power of 40." "At the very instant of totality, the chromosphere at the edge which was the last to be eclipsed—surmounted for a space of about 50° by two groups of prominences, one on the right, the other on the left, of the point of contact—were reproduced in the four spectral lines C, D^3, F, G. My attention was mainly directed to the comparison of the forms of the prominences on the four spectral lines, and I was able to determine that the fundamental form, the skeleton or trunk, and principal branches, were faithfully reproduced or indicated in the images, their extent being, however, greatest in the red, and diminishing successively in the other colours down to the line G, on which the trunk alone was reproduced. In none of the prominences thus compared was I able to distinguish in the yellow image D^3 parts or branches not contained in the red image C. Meanwhile the coloured zones of the corona became continually more marked, one in the red corresponding with the line C, another in the green, probably coinciding with line 1474 K, and a third in the blue, perhaps coinciding with F.

"The green zone surrounding the disk of the Moon was the brightest, the most uniform, and the best defined. The red zone was always distinct and well defined, while the blue zone was faint and indistinct. The green zone was well defined at the summit, though less bright than at the base; its form was sensibly circular and its height about 6′ or 7′. The red zone exhibited the same form and approximately the same height as the green, but its light was weaker and less uniform. These coloured zones shone out upon a faintly illuminated ground without any marked trace of colour. If the corona contained rays of any other kind, their intensity must have been so feeble that they were merged in the general illumination of the field. . . . . The spaces between some of the jets (prominences) were perfectly dark, so that the red zone of the corona appeared to be entirely wanting there. Perhaps, however, this was only an effect of contrast due to the extraordinary brightness of the neighbouring jets.

"The green and red zones were well developed at the western as at the eastern limb, while the blue remained faint and ill-defined. Soon after the appearance of the chromosphere at the western edge, there was suddenly projected on the spectrum of the Sun's limb, which then appeared beyond that of the Moon, a stratum of bright lines, separated by dark spaces, but I could not determine whether they were due to a general or partial reversal of the spectral solar lines, or to a simple discontinuity in the spectrum."

Capt. Tupman, who was stationed at Jaffna, Ceylon, observed the different phenomena as follows:—

"1. The corona extended 40′–50′ from the limb of the Moon.

2. An oblique ray 28′ to 30′ long, two-thirds up a curve ray, like the crook of a boat-hook 12′ long.
3. A sharply-defined rift, with sides forming a right-angle.
4. A narrow rift, very dark, approaching within 2′ or 3′ of the prominences, and still distinct 45′ from them.
5. Another rift opposite to 3.
6. Strong radial polarization with a Savart, extending to 50′ from the Sun. The corona polarization was so strong as entirely to overcome the air polarization. The polarization remained visible as radial upon the corona until 25ˢ, or perhaps 35ˢ, after totality.
7. 1474 K was much the brightest line in the integrating spectroscope. Capt. Fyers saw the four lines C, D^3, 1474 K, and F; also Young's bright reversed lines."

Spectrum Analysis.

Dr. Zöllner has contrived a more simple form of reversion Spectroscope, by which the principle of doubling the change produced in the refrangibility of a line can be applied to any spectroscope. The object-glass of the telescope of the spectroscope is divided, and in front of one half a right-angled prism is placed so that the spectrum seen through it by reflection is reversed. With this instrument Zöllner has succeeded, with the assistance of Dr. Vögel, Director of the observatory at Bothkamp, in detecting the change of refrangibility due to the Sun's rotation. The estimations of the amount of the change subsequently made by Dr. Vögel are in excess of the period of rotation obtained from observations of solar spots, the equatorial velocity ranging from 0·35 to 0·42 mile in a second, while the received value is 0·27 mile. The instrument awaits some minor improvements before reliable quantitative results can be obtained.

Professor Young was fortunate in observing an ejection of matter by an apparent explosion in the Sun, which took place on September 7. At noon on that day he observed an enormous protuberance of hydrogen cloud on the eastern limb of the Sun. It had remained with little change since noon of the preceding day. It was made up mostly of filaments nearly horizontal, with its lower surface at a height of some 15,000 miles; but was connected with the layer of red hydrogen by three or four vertical columns, brighter and more active than the rest. Returning to the telescope half-an-hour later, Professor Young found, in place of the quiet cloud, a mass of detached vertical fusiform filaments rapidly ascending. Some of them had already reached a height of 100,000 miles, and they continued to rise with a motion almost perceptible to the eye until, in ten minutes, the uppermost were more than 200,000 miles above the solar surface, the velocity of ascent was therefore 166 miles per second. We must refer our readers for the details of this important ob-

servation to Professor Young's paper. A notice of some of the conclusions suggested by this observation, as to physics of the Sun, which have been investigated by our fellow Mr. Proctor, will be found in another part of this Report.

Father Secchi has suggested a modification of the usual spectroscopic method of viewing the prominences which may be of service in some observations. He places a prism before the object-glass of the astronomical telescope, or else a direct-vision spectroscope within the telescope, some 6 or 8 inches in front of the slit of the ordinary spectroscope. By this arrangement the Sun's disk can be viewed in the spectroscope; and if the slit be placed on a prominence near the solar limb, and the spectroscope arranged for the position of C, then, together with the solar limb, will be seen a bright line of the prominence at a distance from the Sun's limb, depending on the position of the slit relatively to the solar image, but not exceeding the height of the prominence under observation.

We are also indebted to the ingenuity of Father Secchi for a new method of measuring the height of the prominences seen in the spectroscope, the details of which will be found in the *Comptes Rendus* for December 4, 1871.

Professor Young has done a very valuable service to spectroscopic observers of the Sun by publishing a catalogue of all the lines which have been observed in the solar prominences and corona. These lines, 103 in number, are arranged in the order of their wave-lengths, and the corresponding numbers of Kirchhoff's maps are given. Other columns of the catalogue contain the relative brightness, and frequency of the lines, and the names of the chemical elements to which they belong. No fewer than twenty of the lines are due to the vapour of titanium. Besides the well-known coronal line 1474 of Kirchhoff's scale, two other fainter lines, one less refrangible than C, and the other between F and G, appear, according to Professor Young, to be persistently present in the corona or chromosphere, and probably to be due to the matter of which it consists, and not, as is the case with most of the other lines, to the occasional elevation of matter to heights where it does not properly belong.

The solar prominences have been assiduously observed by the spectroscopic method during the past year by Professor Young, Father Secchi, Professors Respighi and Tacchini. These observations seem to establish the convenient arrangement of these objects into cloud-like and eruption forms; through these observations we are gradually gaining valuable information on many points which are intimately connected with the conditions under which the solar energies are manifested, such as the relation which the prominences sustain to the faculæ and to the spots, the solar zones in which they most frequently present themselves, the mode of their formation, and the density of the solar atmosphere or corona into which they are injected.

Mr. Huggins has succeeded in observing the spectrum of the

planet *Uranus*. He finds the spectrum of *Uranus* continuous, no part being wanting so far as the feebleness of its light permits it to be traced, which is from about C to about G. The absorption taking place at *Uranus* shows itself in six strong lines. One of these, the most refrangible, appeared to be coincident with the line F of hydrogen, with which line (in the spectrum obtained from rarefied hydrogen, rendered luminous by the induction spark) it was directly compared. The positions of this and four other lines were also obtained by micrometrical measures. There is no strong line in the spectrum of *Uranus* in the positions of the strongest lines of air, namely, the double line of nitrogen. There are no absorption bands in the position of the line of sodium, or in that of any line of carbonic acid gas.

Mr. Huggins has also examined spectroscopically the light of Comet I, 1871, discovered by Dr. Winnecke, and of Encke's Comet. Both comets gave a spectrum of three bright bands, agreeing in position with the principal bands of the spectrum of carbon, and similar to the spectra of former comets examined by Mr. Huggins.

Discovery of Minor Planets.

The list of Minor Planets has been increased by five since the last Annual Report. By this addition the total number at the present time is *one hundred and seventeen.* The new members of the group are:—

1. *Amalthæa* (113), discovered, on 1871, March 12, by Dr. R. Luther, at Bilk, near Dusseldorf.
2. *Cassandra* (114), discovered, on July 24, by Dr. C. H. F. Peters, at Hamilton College, Clinton, New York.
3. (115), discovered, on August 6, by Mr. Watson, at Ann Arbor, Michigan.
4. *Sirona* (116), discovered on September 8, by Dr. Peters, at Hamilton College, Clinton. This Planet was independently discovered by Dr. R. Luther on September 14.
5. *Lomia* (117), discovered, on September 12, by M. Borelly, at Marseilles.

It is proper to remark here that several of the minor planets pass through their opposition without being observed on the meridian, for want of an ephemeris. This has been more than usually the case during the past year. The number of planets is now so large that the calculations of ephemerides for all that come into opposition during the year is really a work of considerable labour, and is one which is annually increasing. Hitherto the *Berliner Jahrbuch* has voluntarily undertaken these calculations with great advantage to all who are interested in the observation of the minor planets. A list of the computers who assisted in this work was given in the Annual Report two years ago. The *Berliner Jahrbuch* for 1873 contained the d[illegible]bular places near opposition of fifty-eight minor planets [illegible] but some of the oldest and largest members of the

group were not included among them, and have therefore escaped observation with the meridional instruments of Greenwich and Paris, and probably of other observatories also.

Comets.

Six comets have been visible during the past year. Two of them, Encke's and Tuttle's, were expected to return to perihelion in 1871, but the remaining four appear never to have been observed before. They are here given, named in the order of their discovery or first observation:—

1. Comet I. 1871, discovered, on April 7, by Dr. Winnecke, at Carlsruhe.
2. Comet II. 1871, discovered, on June 14, by M. Tempel, at Milan.
3. Comet III. 1871 (Periodic Comet of Encke), first observed, on October 4, by Dr. Dunér, at Lund. This Comet was first seen in England on October 8, by Mr. Hind.
4. Comet IV. 1871 (Periodic Comet of Tuttle), first observed, on October 12, by M. Borelly, at Marseilles. Dr. Winnecke detected it on October 14.
5. Comet V. 1871, discovered, on November 3, by M. Tempel, at Milan.
6. Comet VI. 1871, discovered, on December 29, by M. Tempel, at Milan.

Encke's comet has been examined by several observers provided with large telescopes. Its discovery was much facilitated by the aid of a daily ephemeris calculated by M. von Glasenapp, of Pulkowa, extending from August 18 to December 18, 1871. The comparison of the first observations with this ephemeris showed that it had at that time an error of 39^s in right ascension, and about $8'$ in declination. A corrected ephemeris was afterwards issued by Mr. Hind. When first seen the comet presented the appearance of a nearly round faint nebulosity, without apparent condensation in any part. By the beginning of November the remarkable fan-like form which has distinguished this appearance of the comet was well marked. A minute stellar nucleus was situated at the eastern point of the fan. On the other side, towards the Sun, which the comet was approaching, the cometary matter was spread out until it was lost to view from faintness. During November the cometary matter on the side from the Sun seemed arrested a short distance behind the apex of the fan where its boundary was seen as a right line perpendicular to the longer axis of the fan-like form. Early in December a faint beam was seen to be projected from this side forming a true tail.

The periodical comet of Tuttle has returned to perihelion, as was expected; but it does not appear to have been observed in England. It has, however, been observed at Marseilles, Carlsruhe, Paris, the Cape, and at a few other places. The positions of Comets I. 1871, II. 1871, and V. 1871, have been well observed. The details of the observations and the elements of each comet are inserted in the *Astronomische Nachrichten.* Elements or ephemerides by Mr. Hind of most of the comets of the year will be found in the *Monthly Notices.*

Two of the prizes offered by the Imperial Academy of Sciences at Vienna for the discovery of telescopic comets have been awarded to Dr. Winnecke for his two new comets discovered in 1870, and one to M. Coggia for the discovery of Comet II. 1870.

Variations of Gravity in Western Russia.

As a necessary supplement to that great geodetical undertaking, the measurement of an arc of the meridian between the Danube and the Arctic Ocean, of which the late Professor Struve was the directing mind, an extensive series of pendulum observations for the determination of the variations of gravity extending throughout the length of that arc, has lately been made under the auspices of the Academy of Sciences of St. Petersburg. The work was intrusted to M. Sawitsch, assisted in 1865 by M. Lenz, and in 1866 and 1868 by M. Smyslof. M. Sawitsch has drawn up a full abstract of the results, which he has forwarded to the Society through the Astronomer Royal. This abstract will be published in the *Memoirs*.

The paper contains the details of the method employed in the observations, and also the resulting values of the length of the seconds pendulum at the twelve different stations, situated on the arc from Tornea in latitude 65° 51′ N., to Ismail in latitude 45° 20′ N, thus ranging over a meridional arc of more than twenty degrees. Two reversible pendulums, constructed with the greatest care by Repsold, were used in the observations. The observed results appear to be very accordant when compared with the corresponding values calculated from formulæ explained by M. Sawitsch in his paper, the sum of the positive residual errors being + 0·0440 Paris lines, while the sum of the negative residuals is — 0·0423 Paris lines. The greatest individual error between the observed and calculated lengths of the pendulum is at Tornea, where it amounts to 0·02 Paris lines. M. Sawitsch remarks that these individual errors depend probably partly upon errors of observation and partly upon anomalies in the intensities of terrestrial gravity.

Catalogue of Comets observed in China.

The volume of *Observations of Comets from* B.C. 611 *to* A.D. 1640, *extracted from the Chinese Annals*, translated from the Chinese by Mr. Williams, and lately published, is a very important contribution towards the ancient history of Astronomy, and one which must be very useful in cometary investigations. It is only as a labour of love that such a work could have been produced, and that such has been the case in the present instance has been very evident to those who have been officially connected with our respected Assistant-Secretary during the preparation of the manuscript for the press. Mr. Williams' attention was first directed to the examination of the old Chinese annals for records of comets in consequence of an application made to him by Mr.

Hind, who had discovered some discrepancy in the path of a comet under discussion by him, contained in M. Biot's Catalogue of Comets observed in China. This led to a systematic examination of Biot's catalogue, which was found to be far more incomplete than was expected, about 150 observations of comets recorded in the under-mentioned works having been omitted by him. The whole of these will be found in this work.

In addition to a detailed Catalogue of the whole of the observations of Comets contained in the Chinese works, known respectively as the Encyclopædia of Ma Twan Lin and the She Ke, Mr. Williams has given a complete set of chronological tables showing the succession of the dynasties and of the emperors from the earliest period to the present time, and other tables to be employed with these for the reduction of Chinese time to European reckoning. The mode of using these tables is fully explained in the introductory remarks, which also contain much information of interest respecting Chinese Astronomy. He also has added a Chinese celestial atlas and reduced copies of the figures in Flamsteed's Atlas, with the principal Chinese asterisms laid down upon the corresponding Stars.

Mr. Williams' complete work in manuscript, containing in every instance the original text in the Chinese characters, has been presented by him to the Society, as mentioned in a former Report, and is therefore available to any Astronomer who may wish to compare the translation with the original Chinese.

Magnitudes of the Stars in the Southern Hemisphere.

The Council have been glad to hear that our Associate, Dr. Gould, has entered upon his labours at Cordoba, Argentine Republic, with every prospect of success. From a communication received from Dr. Gould, dated December 8, we learn that the formal inauguration of the Observatory took place in the autumn, having been delayed by many unexpected obstacles. This interruption to the commencement of the systematic observation of the zones of stars, which was the primary object of the foundation of the observatory, cannot be considered a loss to science, for Dr. Gould and his assistants have not been idle during the time occupied in the construction of the buildings. Following the example of Argelander, who has given us in his *Uranometria Nova* a systematic comparison of the magnitudes of the principal stars in the northern hemisphere, he has undertaken a detailed and laborious series of observations with the naked eye, with the object of forming a catalogue of the magnitudes of all the stars thus visible from 10° north of the equator to the south pole, from which a system of maps could afterwards be constructed representing the aspect of the heavens in those latitudes. On December 8, this important work was far advanced towards completion, and Dr. Gould expects it will be concluded by the time the instruments are ready for regular observation. The number of stars of which the magnitudes are recorded is over 7000, the lowest inserted

being designated 6·6 mag. There can be no doubt that this work of Dr. Gould will be accepted by Astronomers as of the same standard authority for the magnitudes of the stars of the Southern hemisphere, as that of Argelander is admitted to be for those of the Northern.

M. Delaunay's Lunar Tables.

When the Society had the pleasure of presenting its Medal to our distinguished Associate, M. Delaunay, Professor Adams remarked in his address that "the Council have decided that we ought not to await the appearance of M. Delaunay's supplementary researches before we mark emphatically our sense of the value of his labours," adding in a further remark, "I understand, also, that M. Delaunay is engaged in the construction of new Lunar Tables founded upon his theory." The Council are gratified to hear from M. Delaunay that the construction of the new Lunar Tables is now being pushed forward as rapidly as possible. The preparation of the work would have been in a far more advanced state if it had not been for the misfortunes through which the Paris Observatory, in common with other public establishments in that city, had to pass during the siege and the terrible reign of the Commune. M. Delaunay, however, trusts that nothing will now delay the publication of the Tables in a reasonable time from this date.

By the completion of this practical portion of his great work on the Theory of the Moon, M. Delaunay's lengthened theoretical researches will become available for a systematic comparison with observation. That they will stand this test has been fully and ably explained by Professor Adams, in his address on the presentation of the Medal two years ago. The following is the state of the computations at the present time, as reported by M. Delaunay :—"La construction des nouvelles Tables de la Lune est poussée avec toute l'activité possible. Les Tables de la longitude sont complètement terminées, et l'impression en est commencée. Les calculs pour les Tables de la latitude sont en partie effectués (moitié environ) : Les Tables de la parallaxe ne sont pas commencées."

The Greenwich Water-Telescope.

In the last Annual Report allusion was made to this instrument, erected at the Royal Observatory, Greenwich, and destined to the experimental solution of a problem important both in astronomy and physical optics; namely,—Whether any effect is produced on the constant of aberration by the passage of the rays through a refracting medium.

With the bearing of this question on the constitution of the luminiferous ether, astronomers are scarcely concerned; but the

important point to them is, that, should there be any such alteration as has been supposed, the coefficient would vary with the thickness of the object-glass employed, and this to a sensible amount.

The question was raised by Professor Klinkerfues, and has led to an animated discussion, in the course of which MM. Hoek, Sohncke, and Ketteler took exception to Klinkerfues' views, but mainly on theoretical grounds, whilst he supported his conclusions by an experiment, first suggested by Boscovich. The instrument used by him was a diagonal transit of 18 inches focal length, containing a column of oil of turpentine 8 inches long, enclosed between parallel glass plates. The theoretical increase of the aberration in his observations would be about 8″, whilst that observed was 7″·1. It was to test this experiment that the Greenwich instrument was designed by the Astronomer Royal, and the result of last year's observations with it is conclusive.

The observations consisted of measures of the zenith distance of *γ Draconis* near the two equinoxes, where aberration produces the greatest effect, but with opposite signs. On comparing the observed places of the star with the tabular places in the *Nautical Almanac*, in computing which the ordinary coefficient is of course adopted, the latitude of the instrument was determined as follows:—

From the Spring observations	=	51° 28′ 34″·4
From the Autumn observations	=	51 28 33·6

agreeing within the limits of error of observation; while, according to the theory of Professor Klinkerfues, the difference should have been nearly 24″. The observations will be repeated this year.

Scintillation of the Stars.

The results of some very interesting observations made by Professor Respighi, of Rome, on the scintillation of the stars, although not strictly belonging to the work of the past year, deserve a record in the Annual Report of this Society. They form the subject of a paper read before the Accademia Pontificia dei Nuovi Lincei, and published in their *Memoirs*. The observations were made in 1868 and 1869. The instrument employed was an equatoreal by Merz, of 4½ inches aperture, provided with a direct-vision prism by Hofmann, having a cylindrical lens between the prism and the eye-glass. The observations were made on stars in every position, of all magnitudes, and under every variety of circumstances with respect to azimuth, altitude, and atmospheric conditions.

When a star near the horizon was viewed by Professor Respighi through the spectroscope, he observed that its spectrum exhibited the characteristic lines of that star, and also broad bands, usually dark, but occasionally bright, which slowly passed

from one end of the spectrum to the other. Professor Respighi noticed especially that the motion of the undulations was from the red end of the spectrum towards the violet end, for the stars in the west; and, on the contrary, from the violet end to the red end, for the stars in the east; while on or near the meridian the movements were more or less irregular. The undulations were always observed with much more distinctness in stars near the horizon than in those at a great elevation.

From a due consideration of the phenomena presented to his view in the course of his observations, Professor Respighi was led to the conclusion that these undulations, taking into account the opposite movements of the stars in the east and west, were caused not so much by any accidental motion of the atmosphere, as by its general movement ascending towards the west or descending towards the east, as the result of the diurnal motion of the earth. "When the luminous rays," he observes, "traverse the lower regions of the atmosphere, where, in consequence of the differences of temperature and of the unequal condensation of aqueous vapour, the atmosphere itself is found in a state of sensible heterogeneity, the condensed or rarefied atmospheric strata would be brought, by the diurnal rotation of the Earth, for the stars in the west first on the red rays, and afterwards on the more refrangible rays as far as the violet; and, on the contrary, for the stars in the east, at first on the violet rays, and finally on the red. The effect of this is, that the corresponding undulations on the spectrum would travel from the red to the violet for the west stars, and from the violet to the red for the east stars, precisely in the same manner as shown by the observations."

The characteristic phenomena observed by Professor Respighi have been given by him as follows:—

1. In ordinary atmospheric conditions the movement of the bands is from red to violet for all stars in the west, while for stars in the east the motion is from violet to red.

2. For stars on or near the meridian, whether north or south of the zenith, the motion generally oscillates backwards and forwards; sometimes the bands are stationary, or traverse only a very limited portion of the spectrum.

3. Near the horizon the movement of the bands is more irregular and less rapid, while the contrary effect is seen at greater elevations.

4. When the spectroscope is placed in a position so that the spectrum becomes vertical, the motion of the bands is similar to that where the spectrum is horizontal; but the bands are nearly transversal, and not so well defined to an altitude of 30° above the horizon. For greater altitudes they become gradually more indistinct, often changing into longitudinal bands, and occasionally into mere bright or obscure masses.

5. The dark bands are much more frequent than the bright, the latter occurring only near the horizon.

6. In the case of stars of a low elevation, it not unfrequently happens that, in addition to the regular series of bands, others appear less regular in their motion, of greater inclination, and longitudinal.

7. Under the ordinary atmospheric condition, stars near each other present similar phenomena.

8. When the atmosphere is in an abnormal state, the bands are fainter and less decided in their form and motion.

9. During the prevalence of high wind the bands are very indistinct, and little or no motion is perceptible even when the stars are near the horizon.

10. They are also very indistinct when the images of the stars are blurred.

11. When the motion and form of the bands are regular there is generally tranquil weather.

12. The phenomena of scintillation are always most favourably observed on evenings when the atmosphere is charged with an excess of moisture.

Without following Professor Respighi in his discussion of the probable causes by which scintillation is produced, it will perhaps be sufficient to remark that he considers that his observations prove that its origin must be due to the subtraction of a part of the stellar rays by the unequal refraction of the masses of air through which they are made to pass by the diurnal rotation of the Earth; he therefore is inclined to reject the interference theory of Arago, and also that of Montigny, who described the phenomena as arising from the total reflection of a portion of the rays by strata of air unequally heated.

Distribution of the Fixed Stars in Space.

Mr. Proctor has extended to the stars included in Argelander's charts the principle of isographic charting, which he regards as the most effective means at present available for determining the laws according to which the celestial objects are distributed throughout space. These charts contain, as is known, upwards of 324,000 stars, from the first to the eleventh magnitude (Herschel's scale) inclusive, and they present the whole of the northern heavens and a zone two degrees wide south of the equator. The most prominent feature in the isographic chart, as described by Mr. Proctor, is the marked aggregation of stars along the galactic zone. The aggregation is not, however, by any means uniform. It is not towards the central line of that zone that the stars of the above-named orders are gathered; but there are well-defined streams, patches, and nodules of stars, resembling in general respects the complex region of the Milky Way between *Centaurus* and *Aquila*, as pictured by Sir John Herschel. Mr. Proctor considers that, while this result entirely confirms, on the one hand, the general conclusion of the elder Struve that the Galaxy is a region of condensation for the leading

orders of stars, and not alone or chiefly for stars between the 12th and 20th orders; it is, on the other hand, not in accordance with Struve's hypothesis of a central plane, towards which stars of all orders are aggregated. Mr. Proctor regards the relatively narrow cross-section of the streams, patches, and nodules, as demonstrative of the existence of real streams, patches, and nodules of stars in space. Moreover, he points out that it is within the same streams, &c. that Sir W. Herschel found the richest distribution of small stars,—down even, in certain instances, to nebulosity not resolvable with Herschel's largest telescopes; and he hence concludes, that within a certain region of space there coexist stars of all orders of real dimensions, from stars which, at the distance of their proper region, are visible to the naked eye, down to stars so minute and so closely aggregated as to present a nebulous appearance in our most powerful telescopes.

It may, perhaps, be remembered, that in the Supplementary Number of the *Monthly Notices* for 1869, Mr. Proctor presented isographic charts, showing the laws according to which the irresolvable nebulæ are spread over the heavens. He believes that in the outlying streamers of the rich galactic regions in the isographic chart of 324,000 stars, he can recognise a tendency towards those regions of the northern heavens where nebulæ are most abundant. This, of course, would be intelligible, if these nebulæ are in reality not external galaxies, but groups of relatively minute stars, forming part and parcel of our own star-system.

It will be evident that, to establish on a firm basis such theories as these, a more complete and systematic survey of the heavens than has hitherto been attempted should be undertaken. Mr. Proctor advocates the prosecution of such surveys, based on the method of star-gauging, but carried out with telescopes suitably graduated as respects space-penetrating power, and to be eventually extended to the whole of the celestial vault. He considers, that if isographic charts were formed from the gauge-books for the several powers, the comparison of these charts *inter se*, as well as the independent study of each, could not fail to reveal results of the utmost interest and importance.

As a small step on this path, he has begun to gauge the northern heavens with the $4\frac{6}{10}$-inch telescope, No. 3, of the Sheepshanks collection, lent to him by the Society (during the pleasure of the Council) for this purpose.

Motion of Matter projected from the Sun.

In papers which will be found in the *Monthly Notices* for 1870–1871, Mr. Proctor has called attention to evidence suggesting (in his opinion) the existence of disturbances acting vertically with reference to the Sun's globe, and affecting the condition of matter surrounding the Sun. Without actually putting forward the theory that the corona is due to such radial forces,—acting either explosionally or else repulsively without actual explosion,—

he yet considers that such a theory accords better than any other with what is at present known respecting the corona. As evidence in favour of this theory, Mr. Proctor cites the observed great altitude of many eruption prominences (indicative of high velocities of ejection), the greater extension of the inner corona over regions of prominence-activity; the greater extension of the outer corona where the inner is also most extended; the high velocities of certain meteors (which, according to this theory, would be regarded as sun-expelled or star-expelled bodies), such velocities exceeding those due to solar attraction; the chemical constitution of meteorites, their structure, as revealed under the microscope; the radial lines seen in the corona, and other circumstances.

In a later paper (*Monthly Notices*, for December, 1871) Mr. Proctor has discussed the phenomena witnessed by Professor Young on September 7, 1871 (as mentioned elsewhere). He shows that erupted matter could not have traversed the observed space in the observed time, if ejected *in vacuo* from the Sun, unless the velocity of eruption were 260 miles per second, corresponding to a vertical range of 360,000 miles. Since the observed vertical range was but 210,000 miles, he infers that the motion of the hydrogen wisps was excessively retarded by the resistance of the solar atmosphere, and that the actual velocity of eruption was greatly in excess of the above-named amount; and he suggests that denser matter ejected along with (or perhaps through) the glowing hydrogen, would be less retarded, and might retain at least a sufficient velocity to travel to the observed limits of the corona.

Laws of Frequency of Sun-spots.

Messrs. De La Rue, Stewart, and Loewy, have published fresh and very interesting results of their researches into the phenomena of the solar photosphere. The present results relate chiefly to the laws according to which the frequency of Sun-spots attains and descends from the maximum, in different Sun-spot periods. As respects the general law of increase and decrease of spot-frequency, these observers confirm the discovery of Professor Wolf, that the interval of time between a minimum and the next maximum is about one-half as long as the interval between a maximum and the next minimum, the former period being on the average about 3·7 years, the latter about 7·4 years. But as their recognition of this law is based on a much longer and more complete series of observations,—their researches dealing with all Mr. Carrington's observations as well as with the photographic records of the Kew Observatory,—their testimony has a great and independent value. As respects the law of increase and decrease in given spot-periods, their conclusion differs in an important respect from Professor Wolf. The latter appears to consider, that when the spot-frequency has descended rapidly or slowly from a maximum value to the next minimum,

it ascends with corresponding (relative) rapidity or slowness to the next maximum. Messrs. De La Rue, Stewart, and Loewy, consider, on the other hand, that when the spot-frequency has passed rapidly or slowly from a minimum to the next maximum, it descends with corresponding (relative) rapidity or slowness to the next minimum.

The Great Nebula surrounding η Argûs.

Mr. Abbott, of Hobart Town, has invited fresh attention to the subject of the great nebula surrounding *Eta Argûs*. A careful examination of the evidence has led Mr. Lassell to the conclusion that no proof has yet been given of any change in the nebula at all. The Astronomer Royal has also pointed out that Mr. Abbott's observations have not been recorded in a manner which can be regarded as satisfactory. Captain Herschel gives as the result of a careful comparison which he has instituted between Mr. Abbott's drawings and the splendid monograph in Sir John Herschel's *Southern Observations*, that Mr. Abbott appears to have mistaken a low-power view for a small portion of the nebula full of detail. Dr. Gould has compared some observations of the nebula made by him at Cordoba with Sir John Herschel's drawing, and is strongly impressed "with the conviction that the alleged change is altogether imaginary."

Total Eclipses of the Sun, 1872–1900.

So much interest has been taken in all recent total eclipses of the Sun, that it becomes highly important to know the dates when future eclipses of this kind may be expected within the next few years. Those interested in these phenomena are, therefore, much indebted to Mr. Hind for his contribution on this subject, which, it is believed, was first published in the columns of a daily journal. As the results of Mr. Hind's calculations have not appeared in the *Monthly Notices*, it may be useful to future observers to make in this place a more permanent record of a few of the principal facts. It will be seen that during the interval between 1872 and 1900, only one total eclipse of the Sun occurs where the central shadow-path passes over Europe.

Date		The most favourable Locality for Observation.	Greatest Duration of Totality. m s
1874	April 16	South Africa	3 37
1875	April 6	Siam	4 6
1876	September 17	South Pacific	1 40
1878	July 29	W. North America	3 6
1882	May 17	Arabia	2 0
1883	May 6	Marquesas Islands	5 15
1885	September 9	New Zealand	2 0
1886	August 29	West Africa	6 21
1887	August 19	Russia	3 40
1889	December 22	Angola, W. Africa	3 34
1893	April 16	Brazil	4 44

The elements of the eclipse visible in North America on July 29, 1878, are given in detail in the *Monthly Notices* for May, 1871.

Chronographic Determinations of Longitude.

The results of one of the most important of chronographic determinations of longitude have lately been published by Mr. G. W. Dean, of the United States Coast Survey, who was intrusted by Professor Pierce, the Superintendent, with the details of the operation. The terminal stations of this remarkable experiment were Cambridge, Massachusetts, and San Francisco, on the Pacific Coast; the distance between them by the telegraphic route being about 3550 miles. Omaha in Nebraska, and Salt Lake City, were chosen as intermediate stations. The observers were chiefly officers attached to the Coast Survey. Professor Winlock, Director of the Harvard Observatory, superintended the longitude operations at Cambridge. The observations of transits and signals were recorded at each station on a chronograph-register. In addition to the ordinary work for longitude, special experiments were made for the purpose of measuring the "transmission-time" of signals seht direct from Cambridge to San Francisco, and *vice versâ*. The total length of the circuit on these occasions, in the double transmission, was about 7200 miles, using thirteen "telegraph-repeaters," or relay-magnets, in the operation.

It is unnecessary to give in this Report the details of the observations and calculations, which, with the formulæ used in the reductions, are fully given in Mr. Dean's paper. The Fellows will, however, have some notion of the satisfactory character of the results in the following brief abstract of the final observed differences of longitude between Cambridge and San Francisco:—

	Difference of Longitude. h m s	Interval of Time of Transmission through 7200 miles of wire. s
Cambridge to San Francisco direct	= 3 25 7·190	0·817
(C to O) + (O to S. L.) + (S. L. to S. F.)	= 3 25 7·194	0·838
(C to O) + (O to S. F.)	= 3 25 7·183	0·819
(C to S. L.) + (S. L. to S. F.)	= 3 25 7·211	0·833

The excellent agreement of the four independent determinations gives a sufficient guarantee for the trustworthiness of the final mean result. By combining the resulting difference of longitude between Cambridge and San Francisco with that between Greenwich and Cambridge, determined by Dr. Gould in 1866, we have one continuous galvanic measurement of an arc of parallel extending over more than eight hours of longitude.

Another important chronographic determination of the differ-

ence of longitude, of which the results have only recently been published, has been made between a temporary astronomical observatory on the Righi-Kulm and the observatories of Zürich and Neufchâtel, by MM. Plantamour, Wolf, and Hirsch. The observations were made during the summer of 1867, as part of a series of chronographic determinations of the difference of longitude between several places in Switzerland, under instructions given by the Geodesic Commission of that country, who, at their first meeting, decided that these determinations should not be confined to the four observatories of Geneva, Neufchâtel, Berne, and Zürich, but that a certain number of astronomical stations, easily accessible, should be included in the plan of operations, determining at the same time the latitude, azimuth, and intensity of gravity at each of the subordinate stations. One of the results of the series, the difference of longitude between Geneva and Neufchâtel, has already been published.

The results of the present determination of the difference of longitude between the station on the Righi and the observatories of Neufchâtel and Zürich, are given in the greatest detail, and every care appears to have been taken in the arrangement and making of the observations, both of the stars for local time and the registration of the signals. Special attention was also given to the determination of the personal equations between the three observers, which were determined from comparisons of transits observed at intervals from 1861 to 1871. The final results are:—

	m s	
Righi, west of Zürich	0 15·839	Diff. of longitude.
Neufchâtel, west of Righi	6 6·528	„
Neufchâtel, west of Zürich	6 22·367	„

The velocity of the galvanic current was found to be about 7300 miles in a second.

The difference of longitude has also been lately determined between the observatories of Naples and the Collegio Romano at Rome. The details are given in a joint memoir by Father Secchi and Signor Fergola. The signals were transmitted to and from each observatory, on eleven days, between January 2 and 24, 1869, the observations of the stars being recorded on the chronographs at each station. The results are very satisfactory, giving, after the correction for personal equation, a definitive difference of longitude of $7^m\ 6^s{\cdot}247$, the time occupied in the transmission of the galvanic current from one station to the other being $0^s{\cdot}018$.

Tables of Jupiter's Satellites.

Several discussions have lately taken place at the ordinary meetings of the Society respecting the observed inaccuracy of the tabular mean solar times of the eclipses and other phenomena of the satellites of *Jupiter*, computed from Damoiseau's tables,

and published in the *Nautical Almanac.* Although the motions of the satellites are represented by Damoiseau's tables much closer than by the more ancient tables of Delambre, yet a difference of from five to ten minutes is not uncommon between the tabular and observed times of a phenomenon, and occasionally this difference has amounted to more than twenty minutes in transits and occultations of the fourth satellite. The subject is of very great importance, and is one to which the attention of the mathematical astronomer may reasonably be directed at the proper time, but before any new tables can be constructed it will be necessary to accumulate very many more observations of each class of the phenomena, and to compare them with the existing tables. A considerable number of eclipses, &c. have already been observed at various places, especially at the Royal Observatory, Greenwich, since the date of the publication of Damoiseau's tables, but the great majority of them are of the first and second satellites. Of the third satellite the observations are much fewer, and of the fourth they are very rare. They are certainly far too few in number to eliminate the probably large errors of observation, or to sensibly correct the elements adopted by Damoiseau.

In a paper read before the Society at the January meeting, the Astronomer Royal suggests a plan of operation which appears to him to be the only prospect, for very many years to come, of obtaining sufficient observations for the proper construction of new tables. He proposes "that one of our observatories should be permanently devoted to the observation of the phenomena of *Jupiter's* satellites," to be followed up faithfully and systematically. He remarks:—"In late years the observations of the satellites have been greatly neglected. The few observations that have been made of the third and fourth satellites are sufficient to show that the errors of their tables are too large to enable us practically to use these objects for even rude time-determinations, and at the same time are far too rarely determined to enable us to correct the elements of the tables. Of the first satellite there are sufficient observations, possibly also of the second satellite. But it must always be borne in mind that the tabular movements of the satellites cannot be corrected one by one; the necessary relation among their motions, to which I have already alluded, compels us to treat all together, and in combination with one of the most delicate theories of science."

The observed discordances in the times of disappearance and reappearance of the satellites, in and from the shadow of *Jupiter*, noticed in the observations of the same phenomenon made by different observers, depend principally upon the aperture of the telescope employed, but not always. There must, therefore, be some uncertainty in these observations, which can only be eliminated by a large number of comparisons of the observed and tabular times. Delambre, writing in 1817, remarks:—"Pour le quatrième satellite, on s'attend bien que les erreurs seront beaucoup plus fortes que le troisième, et les comparaisons moins

nombreuse. Je trouve entre les observations d'une même éclipse des différences qui vont à 7, 8, 10, 12, et 14 minutes: et ce qu'il y a d'étonnant, c'est que ces discordances énormes ne sont pas toutes vers les limites. J'en trouve une qui est de $29^m\ 15^s$." The number of eclipses of the third and fourth satellites used by Delambre in the construction of his tables were 590 and 334.

The Astronomer Royal believes that any observer undertaking the systematic observation of these phenomena would find "that the mere observations, in their beauty and the incessant variation of their character, would be found very interesting, but that the highest interest would be felt in connecting them with their appropriate theory, and in making the preparations for that numerical work by which each observation would be made to bear its part in the correction of elements and in the ultimate test of the theory."

If one of our existing public or private observatories, furnished with a good equatoreal, would volunteer for this service and carry it out faithfully, the much-wanted accumulation of data would in due time be obtained. Occasional observations at convenient hours are not all that is required; but it should be considered by the observer as a strict part of his duty, that he should record, when practicable, every eclipse, transit, or occultation, and under favourable circumstances, the ingress and egress of the shadows, especially of the third and fourth satellites. Whoever will undertake to make these observations and publish the results in a regular manner, cannot fail to be remembered in the future history of Astronomy, as having made a most valuable contribution to the fundamental data of an interesting and important branch of our science.*

Preparations for the Transit of Venus of 1874.

It is a great pleasure to the Council to be able to report that it has been determined to make photo-heliographic observations of the forthcoming transit of *Venus* in 1874. At the Annual Meeting of the Board of Visitors of the Royal Observatory, in June last, it was resolved that immediate application be made to the Lords Commissioners of Her Majesty's Treasury for a grant of money, in order that a series of photographic records of the transit may be made at the five stations,—Woahoo, Kerguelen's and Rodriguez Islands, Auckland in New Zealand, and Alexandria. The Government, with that liberality which it has shown on other occasions in favour of astronomy, at once granted a sum, not exceeding 5000*l.*, to defray the expenses of construct-

* Since writing the above we are informed that Lord Lindsay intends to undertake the systematic observation of the phenomena of *Jupiter's* satellites at his observatory, Dunecht House, Aberdeen, as soon as the instruments, now being constructed for him, are mounted. He expects to be ready to commence regular observations in about a year from this date. The observatory is situated, however, in too high a latitude for successful observations when *Jupiter* has a large south declination.

ing the necessary instruments. Instructions have therefore been given to Mr. Dallmeyer to construct, under the able supervision of Mr. De La Rue,* five photo-heliographs, of a similar design to that made by him for the Wilna Observatory, and which has produced Sun-pictures of extreme sharpness of definition without sensible distortion. With instruments of this refined class we can look forward to the photographic section of the observations of the transit of *Venus* with great hopes of success. It would have been a great misfortune for science had the expeditions been allowed to leave England unprovided with the means for photographing the different phases of the phenomenon as the planet traverses the Sun's disk from ingress to egress.

It may be remarked, also, that, under the control of the Astronomer Royal, satisfactory progress has been made with the preparation of the instruments and clocks, with which it is intended to furnish each of the five stations, to be used in the direct observation of the ingress and egress, and for the determination of the longitudes and latitudes. Each station will be furnished with three telescopes, a transit-instrument, an altazimuth, and an equatoreal of 6 inches aperture, with their accompanying clocks. For each instrument a separate observing-hut is or will be constructed. All the new instruments have been made by Messrs. Troughton and Simms, and the clocks by Messrs. E. Dent and Co. The telescopes will be supplied with the prismatic eye-piece contrived by the Astronomer Royal for the correction of atmospheric dispersion at low altitudes where some of the observations must necessarily be made. A few of the observing-huts have been put together in the grounds of the Royal Observatory, and a transit-instrument and altazimuth *in situ* were exhibited at the annual visitation in June last. For the convenience of several officers of the Royal Artillery who are expected to form part of the expedition, and to afford them an opportunity for making a practical acquaintance with the instruments, a temporary observatory has been fitted up at Woolwich, near to the officers' quarters.

The Transits of Venus in 2004 *and* 2012.

While the attention of astronomers of various nations is occupied by the necessary preliminary work relating to the transits of *Venus* of 1874 and 1882, Mr. Hind has been making a very interesting calculation for the purpose of showing under what conditions the pair of transits in the beginning of the twenty-first century will take place. M. Le Verrier's Tables of the Sun and *Venus* represent so closely the motions of the

* Besides these five instruments, three more are being constructed of exactly the same form and dimensions; two for the Russian Government and one for Mr. De La Rue, so that there will be eight instruments as alike as possible available for the transit of *Venus*.

Earth and *Venus* in their orbits, that there can be little reason to doubt that the Tables will be as sensibly perfect in 2004 as they are at the present time. Hence, Mr. Hind has every confidence in the results which have been calculated by himself, with every precaution to avoid error. The following is a short abstract of the results, which were first published in the *Proceedings* of the Royal Society.

Transit of 2004.

Greenwich Mean Time of Conjunction in Right Ascension June 7^d 20^h 51^m $28^s.8$.

For the centre of the Earth.

		d	h	m	s		°
First external contact	June	7	17	3	43	at	115·0
First internal contact	„		17	22	35	„	118·0
Second internal contact	„		23	5	40	„	214·6
Second external contact	„		23	24	32	„	218·5

The angles are reckoned from N. towards E. for the direct image.

At Greenwich the entire transit will be visible.

Transit of 2012.

Greenwich Mean Time of Conjunction in Right Ascension = June 5^d 13^h 4^m $44^s.3$.

For the centre of the Earth.

		d	h	m	s		°
First external contact	June	5	10	22	11	at	40·3
First internal contact	„		10	39	56	„	37·8
Second internal contact	„		16	42	6	„	293·1
Second external contact	„		17	0	0	„	290·5

At Greenwich the egress only will be visible, the Sun rising at 15^h 46^m.

The Great Melbourne Telescope.

After the many unexpected difficulties attending the erection and preliminary employment of the Great Melbourne Telescope, it is gratifying to hear that these hindrances—not uncommon, however, in all new instruments when the construction is somewhat of a novel character—have been entirely surmounted, and that the telescope is now in a proper condition for regular and successful observation. The Observatory of Melbourne may therefore be congratulated on having so magnificent a telescope added to its ordinary instruments. An extensive correspondence, containing a complete history of its construction, from its conception to its erection in its appointed home, has been pub-

lished by the Council of the Royal Society. This correspondence includes, among other valuable information, the opinions of persons who are practically acquainted with the use and peculiarities of large astronomical instruments; opinions which cannot fail to be especially valuable to those who may find themselves engaged at some future time on a work of this kind. A great difficulty, and at one time one of great apprehension, arose from the unexpected tarnishing of one of the specula, and it was feared that the repolishing could not be effectively performed at Melbourne. These fears were, however, groundless; for the speculum has not only been repolished, but it has been done to the satisfaction of Mr. Ellery, who speaks of its performance as "highly satisfactory." In his last Report he thus alludes to this instrument:—"Further experience with the great telescope, and of the conditions which affect its performance, have very much enhanced our opinion of it: and the drawings and other results obtained unmistakably show that excellent work can be done with it. On really favourable occasions the performance, even with very high powers, is exceedingly good. It is evident, however, that a telescope of such large dimensions requires a very long practice before it can be fairly or successfully used. The mechanical arrangements are all in perfect order; and the moving and setting the telescope from one object to another is performed almost as quickly and easily as with a five-feet equatoreal. The clock has worked well and with great regularity from the first."

Sir John Herschel's "General History of Double Stars."

It is with great satisfaction and pleasure that the Council inform the Fellows of the Society of a valuable bequest which has been made to it by Sir John Herschel, namely, the gift of the whole of the papers connected with his laborious and most valuable "General History of Double Stars."

Many of the Fellows are probably aware that this great work had occupied a considerable portion of his time and attention for a great many years, and our only regret is that he did not live long enough to complete it, and to see the fruits of his labours. One important portion of it is, however, so nearly complete, that the Council have accepted the recommendation of the Committee appointed to examine and report upon the papers, to print this section entire. The portion of the work referred to is a Catalogue, in order of right ascension, of all the double stars of which Sir John Herschel, in the course of his researches, found observations, amounting in number to about 10,300. For the whole of these stars the approximate right ascensions and north polar distances are supplied for the epoch 1830, and the greater number of the annual precessions for that epoch are filled in. Under the direction of the Committee this great catalogue will be completed and printed; and with regard to the uncompleted portion of the

"General History," which consists of several thousand detached papers, giving the details of the observations of each separate star, for those which Sir John had time to extract from the various catalogues or other authorities which came under his review, the Council have taken measures for their safe custody, and for their being put into such a state as will make them easy for reference by any astronomers who may desire to consult them.

The Fellows will doubtless feel greatly gratified by this proof of the affectionate regard which Sir John Herschel, throughout his long life, entertained for this Society, of which he was one of the chief originators and supporters, and they cannot do otherwise than prize very highly the dedication to it of this, the most laborious probably, if not the greatest, of his works.

The Constant of Precession.

M. Nyrén, of Pulkowa, has made a new and careful determination of the constant of precession, based on the right ascensions contained in Weisse's-Bessel Catalogue of Stars situated between -15° and $+15^\circ$ declination, and in Schejellerup's Catalogue of telescopic stars, also situated between the same degrees of declination. M. Nyrén considers that, although the places given in these catalogues are not very exact, most of the stars having been observed only once, yet he believes that, in the mean results, any uncertainty arising on this account will be balanced by the large number of available comparisons, amounting in round numbers to about 5300. The final results for the general precession obtained by M. Nyrén, reduced to the epoch 1800, is $50''\cdot1882$, differing but slightly from that previously determined.

Trettenero's Catalogue of 1425 Stars.

Volume XV. of the *Memoirs* of the Istituto Veneto di Scienze, Lettere, ed Arti, lately published. contains a Catalogue of the mean right ascensions and declinations of 1425 stars, observed by the late Professor Trettenero with the meridian-circle, at the Observatory of Padua, between April 18, 1861, and February 7, 1863. The stars are confined to a limited zone, extending from the equator to 3° South declination. The observations were reduced by Signor Lorenzoni, successor to Professor Trettenero, whose premature decease took place in May 1863. Professor Santini has affixed a short preface to the Catalogue. Most of the resulting right ascensions and declinations depend upon two observations in both elements; sometimes on as many as four or five, but frequently they depend on one observation only. All the stars appear to be included in the first Catalogue of Weisse's Bessel, and comparisons of the corresponding places of each star are given; the discordances between the two Catalogues being in most cases of no great amount. The series of catalogues of small

stars emanating from the Observatory of Padua, of which the present one is the fifth, is a very valuable contribution to the libraries of our observatories.

Cape Observations.

The first volume of Observations made with the Transit-Circle at the Royal Observatory, Cape of Good Hope, under the direction of Sir Thomas Maclear, has been published during the past year. It contains the separate results for mean R.A. and N.P.D. of stars reduced to January 1, 1856, and observed during that year; a catalogue of the concluded mean R.A. and N.P.D. of the same stars; meridional observations of the Sun, Moon, and planets in 1856; equatorial observations of Encke's Comet in 1855; and detailed tables of the clock and instrumental errors. The second volume is in the press, and will contain the observations made in 1857 and 1858.

We are indebted to Mr. Stone, under whose superintendence these volumes have been prepared for the press, for this first instalment towards the materials for a new standard Catalogue of Southern Stars. He is wisely devoting his attention to the reduction of the meridional observations, which had been yearly accumulating since the erection of the Transit-Circle, before commencing any very elaborate scheme of observation of the small Southern Stars, which he will probably in due time undertake. A considerable number of observations for special purposes have, however, been made during the past year, both with the Transit-Circle and Equatoreal, a reference to which is made in a preceding section of this Report. Mr. Stone has been in communication with the Government with the object of adding a portable altazimuth to the instruments of the Observatory and of improving the mounting of the Equatoreal, which could be made a first-rate instrument of its class, if firmly remounted with all the modern improvements. Mr. Stone is also anxious to have a complete set of magnetical instruments of a similar pattern to those in use at the Kew Observatory.

Pulkowa Observations.

The new volume of the *Observations de Pulkowa*, the third of the series, contains a very elaborate discussion, by M. Wagner, of the resulting mean right ascensions of the principal stars observed with the transit-instrument between the years 1842 and 1852. The observations employed are published in the two preceding volumes of the series. A large portion of the work consists of what may be termed a combined "ledger," exhibiting the separate results for each star observed during the interval, reduced to the general epoch 1845·0, and arranged in groups according to the four positions in which the instrument was placed during the observations. The means of the groups have been taken separately for the four positions, and corresponding corrections have been applied in every case when the star has not been completely

observed, or when the observations made in one position have not been sufficiently numerous. The mean right ascensions deduced from the four groups are in general very accordant with each other, and are an excellent testimony to the care taken by M. Wagner, in his system of calculation. The discordances exhibited, however, between the separate observations are about the same as those found in long groups of resulting right ascensions at other observatories. The second portion of the volume contains a detailed description of the observations made with the Transit-instrument placed in the prime-vertical, and of the methods employed in the instrumental adjustments and in the reduction of the observations. These are divided into three series, of which the first was made by Professor W. Struve in 1840–1842, for the determination of the constant of aberration; the second, by Professor Struve in 1840–1845, and by M. O. Struve in 1858–1859, for the determination of the constant of nutation; while the third series consists of observations made by M. Oom in 1861–1863, who was then attached to the Observatory of Pulkowa.

Papers read before the Society from February 1871, *to February* 1872.

1871.

Mar. 10. Report of the Augusta Eclipse Expedition. Prof. W. G. Adams.
Note on the Corona. Mr. Proctor.
On the Distribution of 326,000 Stars in Argelander's Charts. Mr. Proctor.
On the Colour of the Moon during the late Eclipse. Mr. Proctor.
On the Zodiacal Light. Mr. Ranyard.
Observations of the Solar Eclipse at Greenwich. Mr. Airy.
Erratum in Observations of Solar Eclipse, July 18, 1860. Mr. Airy.
Note on the Misuse of a Common Symbol. Capt. Noble.
Occultation of η *Cancri*, March 3, 1871. Capt. Noble.
Occultation of *Uranus*, March 2, 1871. Capt. Noble.
On a Contrivance for protecting the Observer when a Reflecting Telescope is used in the open air. Mr. Browning.

April 14. Remarkable Appearance during Solar Eclipse, Dec. 1870. Capt. Hardy.
On the Zodiacal Light. Rev. A. Jevons.
On Spectroscope Observations. M. Chacornac.
Observations of *Amalthea* (113). Prof. Luther.
Calculations to obtain the exact Distance of the Sun, Moon, &c., from the Earth. Mr. Dingle.
Observations and Elements of Winnecke's Comet. Mr. Hind.
On the Orbits of ζ *Herculis* and ζ *Cancri*. Mr. Plummer.

May 12. η *Argûs* and the surrounding Nebula. Mr. Abbott.
Occultation of *Uranus*, March 2, 1871. Mr. Maguire.
Note on η *Argûs*. Mr. Tebbutt, jun.
On a Free Regulator Clock. Mr. Kincaid.
Observations of *Mars*. Mr. Joynson.
On a Double Automatic Spectroscope. Mr. Proctor.
On the Total Solar Eclipse, 1878, July 29. Mr. Hind.
Ephemeris of the Periodical Comet of 1867. Mr. Hind.
On the First Comet of 1867. Mr. Hind.
On De Vico's Comet of Short Period. Mr. Hind.
On the Reappearance of Encke's Comet, 1871. Mr. Hind.
Elements and Ephemeris of Comet I. 1871. Mr. Hind.
Une nouvelle Combinaison Spectroscopique. Father Secchi.
On the use of Compound Prisms. Mr. Browning.

On the Changes of Colour in the Equatorial Belt of *Jupiter*. Mr. Browning.

June 9. On the Initial Velocities of the Planets. Mr. Abbatt.

On Auroral and Faint Spectra under small dispersion. Prof. C. P. Smyth.

On the Physical Changes in *Jupiter*. Mr. Ranyard.

On the expression of Delaunay's l, g, h, in terms of finally adopted constants. Prof. Cayley.

Observations of Winnecke's Comet. Rev. S. Perry.

On the Total Solar Eclipse of Dec. 1871, on the Australian Continent. Mr. Hind.

Occultation of 80 *Virginis* by the Moon, May 30, 1871. Capt. Noble.

Nov. 10. On the Precession of the Equinoxes. Mr. Doyley.

Observations of Solar Spots. Mr. Bew.

On supposed Changes in Nebula round η *Argûs*. Mr. Lassell.

Errors in Logarithmic Tables. Mr. Wackerbarth.

Observations of *Saturn*, *Mars*, &c. Rev. J. Spear.

On the Zodiacal Light. Capt. Tupman.

Warning to Spectroscopic Observers. Mr. Drach.

Observations of Encke's Comet. Mr. Airy.

R.A. and N.P.D. of Encke's Comet. Mr. Airy.

Sur la variation de la pesanteur dans les provinces occidentales de l'Empire Russe. M. Sawitsch.

Occultations of Stars by the Moon. Capt. Noble.

Note on the Inferior Conjunction of *Venus*. Capt. Noble.

On the Construction of the Heavens. Mr. Proctor.

Nebuleuses découvertes et observées à Marseille. M. Stéphan.

Dec. 8. Lunar Occultations and Eclipses of *Jupiter's* Satellites. Mr. Tebbutt.

Reply to Queries respecting Observations of η *Argûs*. Mr. Abbott.

On the Spectrum of Hydrogen at Low Pressure. Mr. Seabroke.

On Encke's Comet. Mr. Hollis.

On the Geodesic Lines on an Ellipsoid. Prof. Cayley.

Note on an Universal Equatoreal. Mr. Browning.

Occultation of ι *Capricorni* by the Moon. Capt. Noble.

Eclipse of *Jupiter's* Third Satellite, Dec. 6, 1871. Capt. Noble.

Note on the November Meteors. Capt. Noble.

Note on the Floor of *Plato*. Mr. Birt.

On the Motion of Matter as projected from the Sun. Mr. Proctor.

On Mr. Abbott's imagined Discovery of Changes in *Argo* Nebula. Mr. Proctor.

Note on a special Point in the Determination of the

Moon's Orbit from Meridional Observations of the Moon. Mr. Airy.
Elements of Minor Planet (116). Dr. R. Luther.
Formules pour le Calcul des Orbites des Etoiles doubles. Dr. De Gasparis.
Observations, Elements, and Ephemeris of Tempel's Comet. Mr. Hind.
Elements of Tuttle's Comet for the Southern Hemisphere. Mr. Hind.
Occultation of *Vesta* by the Moon, 1871, Dec. 30. Mr. Hind.

1872.
Jan. 12. On the Variable Star S *Orionis*. Rev. T. W. Webb.
On the Identity of the Triple Star H. i. 13. Mr. Hunt.
Remarks on the Planet *Jupiter*. Mr. Lassell.
A Suggestion on the use of Chronometers. Mr. Gill.
Occultation of *Vesta*, Dec. 30, 1871. Mr. Talmage.
Note on Encke's Comet. Rev. H. C. Key.
Proposed Devotion of a Special Observatory to the Observation of *Jupiter's* Satellites. Mr. Airy.
Observations of Occultations of Stars by the Moon, and Phenomena of *Jupiter's* Satellites. Mr. Airy.
On the Transit of *Mercury*, 12th Nov. 1782, observed by J. Pennington. Rev. A. Freeman.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
Her Majesty's Government in Victoria.
The Lords Commissioners of the Admiralty.
Her Majesty's Secretary of State for India.
Her Majesty's Government, N. S. Wales.
Royal Society of London.
Royal Society of Edinburgh.
Royal Asiatic Society.
Royal Geographical Society.
Royal Institution.
Royal United Service Institution.
Royal Society, N. S. Wales.
Royal Society, Tasmania.
Royal Society, Victoria.
Geological Society.
Photographic Society.
Society of Arts.
Zoological Society.

British Meteorological Society.
The Meteorológical Office.
The Board of Trade.
British Association.
Art-Union of London.
Institute of Actuaries.
British Horological Institute.
London Institution.
The Committee of Civil Engineers.
Literary and Philosophical Society, Leeds.
Literary and Philosophical Society, Liverpool.
Literary and Philosophical Society, Manchester.
Marine Committee, Liverpool.
The Observing Astronomical Society.
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Yorkshire Geological and Polytechnic Society.
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Royal Observatory, Madrid.
Observatory, Berne.
Observatory, San Fernando.
Observatory, Collegio Romano.
Observatory, Zurich.
Observatory, Palermo.
Observatory, Leyden.
Observatory, Sydney.
United States Naval Observatory.
L'Académie des Sciences de l'Institut de France.
Depôt Général de la Marine.
Imperial Academy of Sciences, Vienna.
Imperial Academy of Sciences, St. Petersburg.
Royal Academy of Sciences, Berlin.
Royal Academy of Sciences, Göttingen.
Royal Academy of Sciences, Munich.
Royal Academy of Sciences, Brussels.
Royal Academy of Sciences, Turin.
Hungarian Academy of Sciences, Kasan.
Royal Academy of Sciences, Amsterdam.
Royal Institute of Lombardy.
Royal Society, Copenhagen.
Royal Society, Leipsig.
Astronomische Gesellschaft, Leipsig.
Society of Sciences, Cherbourg.

Academy of Sciences, Batavia.
Physical Society, Berlin.
Academy of Sciences, Bologna.
Royal Society, Upsala.
The University, Upsala.
The Society, Geneva.
United States Government.
United States Naval Department.
United States Bureau of Navigation.
American Philosophical Society.
American Association for the Advancement of Science.
American Academy of Natural Sciences.
Connecticut Academy of Arts and Sciences.
Smithsonian Institution.
George Town College.
Franklin Institute.
Canadian Institute.
Editor of the Athenæum.
Editor of the Student.
Editor of the Quarterly Journal of Science.
Editor of Nature.
Editor of the English Mechanic.
Editors of Silliman's Journal.
Editor of the Revue Scientifique.

G. B. Airy, Esq.
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A. Blaisdell, Esq.
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Prof. Weiss.
Mr. J. Williams.
Dr. R. Wolf.
Prof. C. A. Young.

ADDRESS

Delivered by the President, William Lassell, Esq., on presenting the Gold Medal of the Society to Signor Schiaparelli.

You will have learned from the Report just read, that your Council have awarded the Gold Medal this year to Signor Schiaparelli; and I regret to have to inform you that we shall be deprived of the pleasure of presenting it to him in person; as by a letter received from him a few days ago, I learn that his duties of Professor and Director of the Observatory at Milan will prevent his being able to undertake so long a journey.

The first notice I find of Signor Schiaparelli's labours is his discovery of the minor planet *Hesperia*, at the Observatory of Milan, on the 29th of April, 1861, an indication that, besides his mathematical attainments in Theoretical Astronomy, he possesses industry and practical skill as an observer.

In the *Astronomische Nachrichten* of 13th August, 1864 (No. 1487), is a purely mathematical paper by him, entitled, "Théorèmes sur le mouvement de plusieurs corps qui s'attirent mutuellement dans l'espace." Of this paper, not bearing immediately upon those labours of Signor Schiaparelli which have more especially called forth the award, I will only express the opinion of a friend of high mathematical attainments, who characterizes it "as an elegant and probably original contribution to the theory of the orbits of bodies moving freely in space, and acted on only by their mutual attractions."

I come now to give some account of Signor Schiaparelli's principal discovery of the law of identity of meteors and comets, and of the observations and reflections which led him to that result, as contained in a series of letters to Father Secchi in the year 1866.

It appears from these, that Signor Schiaparelli's study of this subject received a great impulse from his own observation of the meteors which fell on the nights of the 9th, 10th, and 11th of August, 1866. He states that he was then confirmed in the opinion expressed three years before, that, of the meteors which usually fall on those nights, a great number are distinguished by

their starting nearly all from one point. And, from the spasmodic fall of these meteors—more sometimes falling in one minute than in the next quarter of an hour—he inferred that their distribution in space must be very unequal. He also observed that those stars proceeding from one point were all of a fine yellow colour, and left behind them a fugitive but very sensible track; whilst the other meteors, proceeding from various points, offered every variety of colour and form. Hence he concludes that the meteors form a number of rings, and become visible when the Earth traverses their orbit, as if shooting forth from one point in the sky. And he remarks that the observations of M. Coulvier-Gravier, and Professor Heis, and of our own countrymen, Professor Herschel and Mr. Greg, have shown that these radial points occur in every quarter of the heavens: therefore these rings or orbits must possess every possible degree of inclination to the ecliptic.

He then proceeds to inquire how such a mass of cosmical matter could become accumulated in the Solar System. This system seems to consist of two classes—the Planets, characterised by but little excentricity of orbit, slight variation in the plane of the orbit, exclusion of retrograde motion, and a tendency to take the form of a sphere (deviating from it only so much as is necessary to preserve the equilibrium of the body)—these characteristics applying also to the secondary systems, with the exception of the satellites of *Uranus*. The second class consists of cometary bodies, which are under no law as to the planes of their orbits, or the direction of their motions. The point most remarkable about them is the extreme elongation of their orbits, most of which are described in stellar space; which seems to show that they did not form part of our system when that was first constituted, but are wandering nebulæ picked up by our Sun.

Signor Schiaparelli further observes that the velocity of the Solar System through space has been shown by Otto Struve and Airy to be somewhat similar to that of the planets round the Sun. Now if a nebulous body or comet in motion were to come within the action of the Sun, it would go round the Sun at such an immense distance from us, that it would remain invisible. Two circumstances might bring it within our range of vision—first, if the comet met the Sun in almost a direct line; and secondly, if it were travelling in a direction parallel to the Sun's motion.

If we suppose a cloud of cosmical matter formed of particles so minute and so widely separated, as to possess scarcely any mutual attraction, to be brought within the power of the Sun's influence, each particle would pursue an elliptic orbit of its own. Those particles which differed most in the *planes* of their orbits, would however possess *nodes in common*, and, in consequence, the particles as they approached the Sun would necessarily approach each other, and when separating again, after passing the node, would at their perihelion passage be still very much nearer than they were when brought first within the Sun's attraction. Those particles which, lying in the same plane, pre-

sented a wide angle with respect to the Sun, would form *ellipses*, the planes of which would be identical; though the positions of the major axes would diverge: and, as a result, the particles at their perihelion would pass in nearly the same orbit, but at different velocities, the originally foremost particle being overtaken by those behind it. Again, those particles which, being in the same *plane*, were also in the same *line with regard to the Sun*—their separation consisting in the variation of their distance from the Sun—would form *ellipses in the same plane*, and having a major axis in the same direction, but of different lengths,—the orbit of the particle *nearest* the Sun being described *within* that of the furthest particle, the result of which would be a difference of speed, and an ever-widening distribution of the particles along the whole of the orbit. This reasoning is illustrated, in the second letter to Father Secchi, by a series of diagrams and figures; and then Signor Schiaparelli proceeds to give a recapitulation or summary of his principal propositions thus:—Celestial matter may be divided into the following classes, 1st, fixed stars; 2nd, agglomerations of small stars (resolvable nebulæ); 3rd, smaller bodies invisible except when approaching the Sun (comets); 4th, small particles composing a cosmical cloud. This last class probably occupies a large portion of the celestial spaces, and the motion of these dust-clouds may be similar to that of the fixed stars. When attracted by the Sun they are not visible unless they receive an orbit which is an elongated conic section.

Whatever may have been the original form of the cloud, it cannot penetrate far into our system without assuming the form of an elongated cylinder passing gradually into a stream of particles. The number of such streams seems to be very great. The particles are so scattered that their orbits may cross each other without interruption and may possibly be always changing like the beds of rivers. The stream, after passing its perihelion, will be more diffuse than before; and, when passing a planet may be so violently affected as to separate or break up and even some particles may assume quite a new orbit and become independent meteors.

Thus meteors and other celestial phenomena of like nature, which a century ago were regarded as atmospheric phenomena —which La Place and Olbers ventured to think came from the Moon, and which were afterwards raised to the dignity of being members of the planetary system—are now proved to belong to the *stellar regions*, and to be *in truth—falling stars*. They have the same relation to comets as the asteroids have to the planets; in both cases their small size is made up by their greater number.

Lastly, we may presume that it is certain that falling stars, meteors, and aerolites, differ in size only and not in composition; therefore we may presume that they are an example of what the universe is composed of. As in them we find no elements foreign to those of the Earth, we may infer the similarity of composition

of all the universe—a fact already suggested by the revelations of the spectroscope.

Signor Schiaparelli further pursues the subject in another and later paper, published in No. 1629 of the *Astronomische Nachrichten*, entitled, "Sur la Relation qui existe entre les Comètes et les étoiles filantes." In this communication he refers to the letters to Father Secchi above referred to, in which he had endeavoured to bring together all the arguments in favour of the opinion of an analogy between the mysterious bodies known as shooting stars and comets.

Signor Schiaparelli, in this paper, proceeds to state, that he is prepared to afford to this analogy a large amount of probability, since there is no doubt that certain comets, if not all, furnish the numerous meteors which traverse the celestial spaces. In proof of this, Signor Schiaparelli quotes from a paper of Professor Erman, in which he has pointed out the method of obtaining a complete knowledge of the orbit described by a system of shooting stars, when the apparent position of the point of radiation and the velocity through space of the meteors is known.

Assuming from the necessity of the case that the orbit of the August meteors must be an elongated conic section, Signor Schiaparelli employs the method of Erman to calculate the parabolic orbit of those bodies; taking right ascension 44° and north declination 56° for the position of the point of divergence, according to the observations made in 1863 by Professor A. S. Herschel. And he proceeds to give the following elements, assuming the maximum of the display of 1866 to be August 10th, 18 hours. Comparing these elements of the orbit of the August meteors with those of the orbit of Comet II. 1862, calculated by Dr. Oppolzer, he exhibits the following remarkable coincidence in each element:—

	Elements of the Orbit of the August Meteors.	Elements of the Orbit of Comet II. 1862.
Perihelion Passage	23 July, 1862	22·9 August, 1862
Longitude of Perihelion	343° 28′	344° 41′
Ascending Node	138 16	137 27
Inclination	64 3	66 25
Perihelion Distance	0·9643	0·9626
Revolution Period	105? years	123·4 years
Motion	Retrograde.	Retrograde.

Although the time of revolution of the August meteors is still doubtful, Signor Schiaparelli, on reference to the catalogues of Biot and Quetelet, deduces a hypothetic period of 105 years, which introduces but small changes in the elements—very inferior to the uncertainty of some of the data on which this determination is built.

In the letters above referred to, Signor Schiaparelli had given an orbit for the meteors of November, assuming the point of radiation as determined in America to be γ *Leonis*. But later observa-

tions made with much care in England have shown that this position is erroneous by several degrees; so that that orbit can only be termed a very rough approximation. Assuming, then, that the point of radiation is longitude 143° 12′ and latitude 10° 16′ north—that the maximum of the shower was Nov. 13th, 11h. G.M.T.—and that the period of revolution is 33¼ years, according to Professor Newton—Signor Schiaparelli computed the following elements of the meteoric orbit, which he compared with those of the orbit of Comet I., 1866, calculated by Dr. Oppolzer.

	Elements of the Orbit of the November Meteors.	Elements of the Orbit of Comet I. 1866.
Perihelion Passage	Nov. 10·092, 1866.	Jan. 11·160, 1866.
Longitude of the Perihelion	56° 25·9	60° 28
Ascending Node	231 28·2	231 26·1
Inclination	17 44·5	17 18·1
Perihelion Distance	0·9873	0·9765
Eccentricity	0·9046	0·9054
Semi-axis Major	10·340	10·324
Revolution Period	33·250 years	33·176 years.
Motion	Retrograde	Retrograde.

The assumed position of the point of radiation of the meteors is the mean of 15 determinations obtained by Professor A. S. Herschel and given in the *Monthly Notices* of our Society, vol. xxvii. page 1[illegible]. If this point be advanced 2 degrees in longitude, and 145° be taken in lieu of 143°, the difference of 4 degrees in the place of the longitude of perihelion in the above elements will disappear.

Signor Schiaparelli then concludes his memoir in these remarkable words: "These approximations need no comment—must we regard these falling stars as swarms of small comets, or rather as the product of the dissolution of so many great comets? I dare make no reply to such a question."

In venturing to offer a word or two of comment on this very important branch of the labours of Signor Schiaparelli, it appears to me that we can scarcely speak of them too highly, or overrate their importance. Granting that his hypotheses are correct—of which indeed there seems to be a very high probability, some of the most difficult questions in the contemplation of the constitution of the universe seem at once, and as it were *per saltum*, to be solved. To have placed before our view so clear a history of these mysterious bodies—nebulæ, comets, and meteors, and their several and intimate relations pointed out—is an advancement of Astronomical Science I at least individually had not ventured to anticipate. And a collateral advantage resulting from this splendid discovery, is the encouragement given to the careful and diligent observation of phenomena, even when the prospect of a fruitful result is by no means apparent. Had it not been for the patient, systematic, and intelligent observations of Professor Heis

M. Coulvier-Gravier, Mr. Greg, and Professor Herschel, Signor Schiaparelli would have wanted many valuable data required in his investigations.

I may finally remark, that an important confirmation of Signor Schiaparelli's conclusions appears in a valuable paper of Professor Adams, in our *Monthly Notices*, vol. xxvii. p. 247, in which from somewhat different data, including some observations of his own, he calculates elliptic elements of the November meteors generally very accordant with those above given.

The President then delivered the Medal to the Foreign Secretary on behalf of Signor Schiaparelli, addressing him in the following terms :—

Colonel Strange, — I have the pleasure to present to you the Gold Medal of the Society, which please to receive on behalf of Signor Schiaparelli, and transmit it to him with our best wishes that he may enjoy a long life of successful research and discovery.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

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A. Cayley, Esq. M.A. F.R.S., Sadlerian Professor of Geometry, Cambridge.

Vice-Presidents:

J. C. Adams, Esq. M.A. F.R.S., Lowndean Professor of Astronomy, Cambridge.
Warren De La Rue, Esq. D.C.L. F.R.S.
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Samuel Charles Whitbread, Esq., F.R.S.

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Captain William Noble.
Rev. Charles Pritchard, M.A. F.R.S., Savilian Professor of Astronomy, Oxford.
A. Cowper Ranyard, Esq. M.A.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXII. *March* 8, 1872. No. 5.

Professor Cayley, President, in the Chair.

Rev. Samuel Jenkins Johnson, Upton Helions, near Crediton;
George Walter Roberts, Esq., Gomersal, near Leeds;
Alfred Herrtage, Esq., University College,

were balloted for and duly elected Fellows of the Society.

On a Pair of Differential Equations in the Lunar Theory.
By Prof. Cayley.

I consider the differential equations

$$\frac{d}{dt}\frac{d\varrho}{dt} - \varrho\left(\frac{dv}{dt}\right)^2 + \frac{1}{\varrho^2} = k m^2 \varrho \left\{\frac{1}{2} + \frac{3}{2}\cos(2v - 2mt)\right\},$$

$$\frac{d}{dt}\left(\varrho^2 \frac{dv}{dt}\right) \qquad = j m^2 \varrho^2 \left\{ \quad -\frac{3}{2}\sin(2v - 2mt)\right\},$$

which when $j = k = 1$ give the following equations in the lunar theory ($D = t - mt$):

$$\frac{1}{\varrho} = 1 + \frac{1}{6}m^2 - \frac{179}{288}m^4 - \frac{97}{48}m^5 - \frac{757}{162}m^6 - \frac{4039}{432}m^7 - \frac{34751189}{1990656}m^8$$
$$- \frac{155067635}{4976640}m^9$$

$$+\cos 2\,\mathrm{D}\left[m^2 + \frac{19}{6}m^3 + \frac{131}{18}m^4 + \frac{383}{27}m^5 + \frac{510565}{20736}m^6 + \frac{23140781}{622080}m^7 + \frac{355021217}{9331200}m^8 + \frac{27888590059}{34992000}m^9\right]$$

$$+\cos 4\,\mathrm{D}\left[\frac{7}{8}m^4 + \frac{2737}{480}m^5 + \frac{162869}{7200}m^6 + \frac{7554833}{108000}m^7 + \frac{23894167223}{12960000}m^8 + \frac{2335230125283}{5443200000}m^9\right]$$

$$+\cos 6\,\mathrm{D}\left[\frac{219}{256}m^6 + \frac{151339}{17920}m^7 + \frac{29887443}{627200}m^8 + \frac{98978623957}{444528000}m^9\right],$$

$$+\cos 8\,\mathrm{D}\left[\frac{2701}{3072}m^8 + \frac{70033633}{6021120}m^9\right],$$

or as far as m^7,

$$\rho = 1 - \frac{1}{6}m^2 + \frac{331}{288}m^4 + \frac{83}{16}m^5 + \frac{42775}{2592}m^6 + \frac{4787}{108}m^7$$

$$+\cos 2\,\mathrm{D}\left[-m^2 - \frac{19}{6}m^3 - \frac{125}{18}m^4 - \frac{709}{54}m^5 - \frac{485173}{20736}m^6 - \frac{24487949}{622080}m^7\right]$$

$$+\cos 4\,\mathrm{D}\left[-\frac{3}{8}m^4 - \frac{1217}{480}m^5 - \frac{74069}{7200}m^6 - \frac{1749779}{54000}m^7\right]$$

$$+\cos 6\,\mathrm{D}\left[-\frac{59}{256}m^6 - \frac{126193}{53760}m^7\right],$$

($\frac{1}{\rho}$ is given by M. Delaunay only as far as m^5, the additional terms of $\frac{1}{\rho}$ and expression for ρ were kindly communicated to me by Prof. Adams); and

$$v = t$$

$$+\sin 2\,\mathrm{D}\left(\frac{11}{8}m^2 + \frac{59}{12}m^3 + \frac{893}{72}m^4 + \frac{2855}{108}m^5 + \frac{8304449}{165888}m^6 + \frac{102859909}{1244160}m^7 + \frac{7596606727}{74649600}m^8 - \frac{8051418161}{111974400}m^9\right)$$

$$+\sin 4\,\mathrm{D}\left(\frac{201}{256}m^4 + \frac{649}{120}m^5 + \frac{647623}{28800}m^6 + \frac{31363361}{432000}m^7 + \frac{123030377303}{414720000}m^8\right)$$

$$+ \sin 6\,D \left(\frac{3715}{6144} m^6 + \frac{664571}{107520} m^7\right)$$

(Delaunay, t. xi. pp. 815, 836, 845.)

To integrate the original equations write

$$\begin{aligned} \rho &= 1 + \rho_1 + \rho_2 + \dots \\ v &= t + v_1 + v_2 + \dots \end{aligned}$$

where the suffixes indicate the degrees in the co-efficients k, j conjointly: the equations for ρ_n, v_n take the form

$$\frac{d}{dt}\frac{d\rho_n}{dt} - 3\rho_n - 2\frac{dv_n}{dt} + V_n = Q_n,$$

$$\frac{d}{dt}\left(\frac{dv_n}{dt} + 2\rho_n + U_n\right) \qquad = P_n,$$

where V_n, U_n, P_n, Q_n do not contain ρ_n or v_n. From the second equation we have

$$\frac{dv_n}{dt} + 2\rho_n + U_n = \Omega_n + \int P_n\, dt,$$

where Ω_n is a constant of integration, the integral $\int P_n\, dt$ containing only periodic terms; and then adding twice this to the first equation we have

$$\frac{d}{dt}\frac{d\rho_n}{dt} + \rho_n + V_n + 2U_n = 2\Omega_n + Q_n + 2\int P_n\, dt$$

which determines ρ_n; and substituting its value in the other equation we have $\frac{dv_n}{dt}$, and thence v_n; the constant Ω_n is determined so that $\frac{dv_n}{dt}$ may contain no constant term. We have

$$_1 = 0,$$

$$_2 = -\left(\frac{dv_1}{dt}\right)^2 - 2\rho_1\frac{dv_1}{dt} + 3\rho_1^2,$$

$$_3 = -2\frac{dv_1}{dt}\frac{dv_2}{dt} - 2\rho_1\frac{dv_2}{dt} - \rho_1\left(\frac{dv_1}{dt}\right)^2$$

$$- 2\rho_2\frac{dv_2}{dt} + 6\rho_1\rho_2 - 4\rho_1^3,$$

t

$$U_1 = 0,$$

$$U_2 = 2\rho_1\frac{dv_1}{dt} + \rho_1^2,$$

$$U_3 = 2\rho_1\frac{dv_2}{dt} + (2\rho_2 + \rho_1^2)\frac{dv_1}{dt} + 2\rho_1\rho_2,$$

&c.

$$Q_1 = k m^2 \left(\frac{1}{2} + \frac{3}{2} \cos 2 D \right),$$

$$Q_2 = k m^2 \left\{ 3 v_1 \sin 2 D + \rho_1 \left(\frac{1}{2} + \frac{3}{2} \cos 2 D \right) \right\},$$

$$Q_3 = k m^2 \left\{ - 3 v_2 \sin 2 D - 3 v_1^2 \cos 2 D \right.$$
$$+ \rho_1 v_1 \cdot 3 \sin 2 D$$
$$\left. + \rho_2 \left(\frac{1}{2} + \frac{3}{2} \cos 2 D \right) \right\},$$

&c.

$$P_1 = j m^2 \left(- \frac{3}{2} \sin 2 D \right),$$

$$P_2 = j m^2 (- 3 v_1 \cos D - 3 \rho_1 \sin 2 D)$$

$$P_3 = j m^2 \left\{ - 3 v_2 \cos 2 D + 3 v_1^2 \sin 2 D, \right.$$
$$- 6 \rho_1 v_1 \cos 2 D$$
$$\left. + (2 \rho_2 + \rho_1^2) \cdot - \frac{3}{2} \sin 2 D \right\}$$

&c.

In particular attending to the values of P_1, Q_1 the equations for ρ_1, v_1 are in their original form

$$\frac{d}{dt} \frac{d \rho_1}{dt} - 3 \rho_1 + 2 \frac{d v_1}{dt} = k m^2 \left(\frac{1}{2} + \frac{3}{2} \cos 2 D \right),$$

$$\frac{d}{dt} \left(\frac{d v_1}{dt} + 2 \rho_1 \right) \quad = j m^2 \left(- \frac{3}{2} \sin 2 D \right),$$

whence in the transformed form they are

$$\frac{d v_1}{dt} + 2 \rho_1 = \Omega_1 + \frac{3 j m^2}{4 (1 - m)} \cos 2 D,$$

and

$$\frac{d^2 \rho_1}{d t^2} + \rho_1 = 2 \Omega_1 + k m^2 \left(\frac{1}{2} + \frac{3}{2} \cos 2 D \right) + \frac{\frac{3}{2} j m^2}{1 - m} \cos 2 D.$$

Thus the constant term of ρ_1 is $2 \Omega_1 + \frac{1}{2} k m^2$, giving in $\frac{d v_1}{dt}$ a constant term $- 3 \Omega_1 - k m^2$; this must vanish, or we have $\Omega_1 = - \frac{1}{3} k m^2$; and the equations thus become

$$\frac{d v_1}{dt} + 2 \rho_1 = - \frac{1}{3} k m^2 + \frac{3 j m^2}{4 (1 - m)} \cos 2 D,$$

$$\frac{d^2 \rho_1}{d t^2} + \quad \rho_1 = - \frac{1}{6} k m^2 + \left(\frac{3}{2} k m^2 + \frac{\frac{3}{2} j m^2}{1 - m} \right) \cos 2 D,$$

and then completing the integration

$$\rho_1 = - \frac{1}{6} k m^2 + \left\{ \frac{- \frac{3}{2} k m^2}{3 - 8 m + 4 m^2} + \frac{- \frac{3}{2} j m^2}{(1 - m)(3 - 8 m + 4 m^2)} \right\} \cos 2 D,$$

$$v_1 = \qquad \left\{ \frac{\frac{3}{2} k m^2}{(1 - m)(3 - 8 m + 4 m^2)} + \frac{\frac{3}{8} j m^2 (7 - 8 m + 4 m^2)}{(1 - m)^2 (3 - 8 m + 4 m^2)} \right\} \sin 2 D.$$

which are the accurate values of ρ_1 and v_1.

Expanding as far as m^6 we have

$$\rho_1 = k\left(-\tfrac{1}{6}m^2\right) + \cos 2\mathrm{D}\left\{ k\left(-\tfrac{1}{2}m^2 - \tfrac{4}{3}m^3 - \tfrac{26}{9}m^4 - \tfrac{160}{27}m^5 - \tfrac{968}{81}m^6\right)\right.$$

$$\left. + j\left(-\tfrac{1}{2}m^2 - \tfrac{11}{6}m^3 - \tfrac{85}{18}m^4 - \tfrac{575}{54}m^5 - \tfrac{3661}{162}m^6\right)\right\}$$

which for $j = k$ is $$= k\left(-\ m^2 - \tfrac{19}{6}m^3 - \tfrac{137}{18}m^4 - \tfrac{895}{54}m^5 - \tfrac{5597}{162}m^6\right)$$

and

$$v_1 = \sin 2\mathrm{D}\left\{ k\left(\tfrac{1}{2}m^2 + \tfrac{11}{6}m^3 + \tfrac{85}{18}m^4 + \tfrac{575}{54}m^5 + \tfrac{3661}{162}m^6\right)\right.$$

$$\left. + j\left(\tfrac{7}{8}m^2 + \tfrac{37}{12}m^3 + \tfrac{589}{72}m^4 + \tfrac{1037}{54}m^5 + \tfrac{27331}{648}m^6\right)\right\}$$

which for $j = k$ is $$= k\left(\tfrac{11}{8}m^2 + \tfrac{59}{12}m^3 + \tfrac{929}{72}m^4 + \tfrac{896}{27}m^5 + \tfrac{41975}{648}m^6\right).$$

I have, not in general, but for the value $j = k$, calculated ρ_2 and v_2 as far as m^6: I have not made the calculation for ρ_3 and v_3, but their values may be deduced from the foregoing values of ρ, v; the final expressions (when $j = k$) of $\rho, = 1 + \rho_1 + \rho_2 + \rho_3 + \ldots$ and $v, = t + v_1 + v_2 + v_3 \ldots$ are

$$\begin{aligned}
\rho = 1 \quad & \\
& + k\left(-\tfrac{1}{6}m^2\right) \\
& + k^2\left(\tfrac{331}{288}m^4 + \tfrac{83}{16}m^5 + \tfrac{5113}{288}m^6\right) \\
& + k^3\left(-\tfrac{1621}{1296}m^6\right) \\
+ \cos 2\mathrm{D}\Big\{ & \ k\left(-\ m^2 - \tfrac{19}{6}m^3 - \tfrac{137}{18}m^4 - \tfrac{895}{54}m^5 - \tfrac{5597}{162}m^6\right) \\
& + k^2\left(\tfrac{2}{3}m^4 + \tfrac{31}{9}m^5 + \tfrac{329}{27}m^6\right) \\
& + k^3\left(-\tfrac{2381}{2304}m^6\right)\Big\} \\
+ \cos 4\mathrm{D}\Big\{ & \ k^2\left(-\tfrac{3}{8}m^4 - \tfrac{1217}{480}m^5 - \tfrac{76589}{7200}m^6\right)
\end{aligned}$$

$$+k^3\left(\qquad\qquad\qquad\qquad + \frac{7}{20}\, m^6\right)\Big\}$$

$$+\cos 6\,D\,\Big\{\; k^3\left(\qquad\qquad\qquad\qquad - \frac{59}{256}\, m^6\right)\Big\}.$$

and

$$v = t$$

$$+\sin 2\,D\,\Big\{\; k\left(\frac{11}{8}m^2 + \frac{59}{12}m^3 + \frac{929}{72}m^4 + \frac{896}{27}m^5 + \frac{41975}{048}\, m^6\right)$$

$$+k^2\left(\qquad\qquad - \frac{1}{2}\, m^4 - \frac{41}{12}\, m^5 - \frac{43}{3}\, m^6\right)$$

$$+k^3\left(\qquad\qquad\qquad\qquad - \frac{783}{2048}\, m^6\right)\Big\}$$

$$+\sin 4\,D\,\Big\{\; k^2\left(\qquad \frac{201}{256}m^4 + \frac{649}{120}m^5 + \frac{665263}{28800}m^6\right)$$

$$+k^3\left(\qquad\qquad\qquad\qquad - \frac{49}{80}\, m^6\right)\Big\}$$

$$+\sin 6\,D\,\Big\{\; k^3\left(\qquad\qquad\qquad\qquad + \frac{3715}{6144}\, m^6\right)\Big\};$$

which for $k = 1$ agree with the foregoing formulæ (verifying them as far as m^5); the present formulæ exhibit the manner in which the expressions depend on the several powers of the disturbing force.

On the Variations of the Position of the Orbit in the Planetary Theory. By Prof. Cayley.

It has always appeared to me that in the Planetary Theory, more especially when the method of the variation of the elements is made use of, there is a difficulty as to the proper mode of dealing with the inclinations and longitudes of the nodes, hindering the ulterior development of the theory. Considering the case of two planets m, m', and referring their orbits to any fixed plane and fixed origin of longitudes therein, let θ, θ' be the longitudes of the nodes, ϕ, ϕ' the inclinations ($p = \tan\phi \sin\theta$, $q = \tan\phi\cos\theta$, &c., as usual); then the disturbing functions for m, m' respectively are developed, not explicitly in terms of ϕ, ϕ', θ, θ', but in terms of Φ, the mutual inclination of the two orbits, and of Θ, Θ' the longitudes in the two orbits respectively of the mutual node of the two orbits; Φ and Θ, Θ' being functions (and complicated

ones) of ϕ, ϕ', θ, θ'. Moreover, although in the general theory of the secular variations of the orbits of the planetary system, θ, ϕ, &c., are, as above, referred to one fixed plane (the ecliptic of a certain date), yet in the theory of each particular planet it is the practice, and obviously the convenient one, to refer for such planet the θ, ϕ to its own fixed plane (the orbit of the planet at a certain date), the effect of course being that ϕ, and consequently p, q, instead of being of the order of the inclinations to the ecliptic, are only of the order of the disturbing forces. It has occurred to me that the last-mentioned plan should be adhered to *throughout;* viz., that for each planet m, the position of its variable orbit should be determined by θ, the longitude of its node, and ϕ, the inclination in reference to the appropriate fixed plane (orbit of the planet at a certain date) and origin of longitude therein. The disturbing functions for the planet m and m' will of course depend not only on θ, θ', ϕ, ϕ', but on the quantities Φ, Θ, Θ' which determine the mutual positions of the two fixed planes of reference and origins of longitude therein, *these last being however absolute constants not affected by any variation of the elements;* so that as regards the variation of the elements the disturbing functions are in fact given as *explicit* functions of the variable elements θ, θ', ϕ, ϕ'; and where ϕ, ϕ' and therefore also p, q, p', q' are only of the order of the disturbing forces.

I proceed to work out this idea, for the present considering the development of the Disturbing Function only as far as the first powers of p, q &c. For comparison with the ordinary theory, observe that in this theory the disturbing function contains only the *second* powers of the p, q &c., made use of therein; these are in fact of a form such as $P + p$, $Q + q$, ... where P, Q are absolute constants and p, q ... are the p, q, ... of the present theory; the ordinary theory gives therefore in the disturbing function a series of terms involving $(P + p)^2$, $(P + p)(Q + q)$, ... which I now take account of only as far as the first powers of p, q, ... viz., they are in effect reduced to $P^2 + 2Pp$, $PQ + Pq + Qp$, &c.... The present theory is thus not now developed to the extent of giving the p, q ... of the ordinary theory in the more complete form as the solutions of a system of simultaneous linear differential equations, but only to the extent of obtaining for these p, q ... respectively the terms which are proportional to the time.

I commence with the following subsidiary problem. Consider a spherical triangle A B C (sides a, b, c, angles A, B, C, as usual), and taking the side c as constant, but the angles A and B as variable, let it be required to find the variations of C, a, b in terms of variations dA, dB and the variable elements C, a, b themselves. Although the geometrical proof would be more simple, I give the analytical one, as it may be useful.

We have

$$\cos C = -\cos A \cos B + \sin A \sin B \cos c,$$

and thence

$$
\begin{aligned}
-\sin C\, dC = {} & (\sin A \cos B + \cos A \sin B \cos c)\, dA \\
& + (\sin B \cos A + \sin A \cos B \cos c)\, dB \\
= {} & \frac{\sin B \sin c}{\tan b}\, dA + \frac{\sin A \sin c}{\tan a}\, dB,
\end{aligned}
$$

that is

$$
-\frac{\sin C}{\sin c}\, dC = \frac{\sin B \cos b}{\sin b}\, dA + \frac{\sin A \cos a}{\sin a}\, dB,
$$

or finally,

$$
-dC = \cos b\, dA + \cos a\, dB.
$$

Next

$$
\sin a = \sin c \frac{\sin A}{\sin C},
$$

or differentiating

$$
\cos a\, da = \frac{\sin c}{\sin^2 C}(\sin C \cos A\, dA - \cos C \sin A\, dC),
$$

or substituting for dC its value,

$$
\begin{aligned}
= {} & \frac{\sin c}{\sin^2 C}\Big\{ dA\,(\sin C \cos A + \cos C \sin A \cos b) \\
& \qquad + dB \,.\, \cos C \sin A \cos a \Big\} \\
= {} & \frac{\sin c}{\sin^2 C}\Big\{ dA\, \frac{\sin A \sin b \cos a}{\sin a} \\
& \qquad + dB \cos C \sin A \cos a \Big\},
\end{aligned}
$$

that is,

$$
da = \frac{1}{\sin C}\left\{ dA \frac{\sin A}{\sin a} \sin b + dB \cos C \sin A \right\} \div \frac{\sin C}{\sin c},
$$

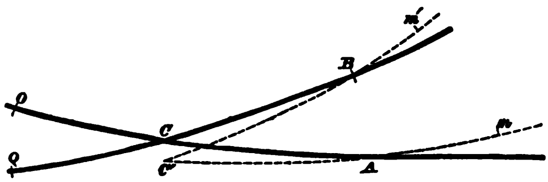
or on the right-hand writing $\frac{\sin A}{\sin a}$ instead of $\frac{\sin C}{\sin c}$ this is

$$
da = \frac{1}{\sin C}(dA \sin b + dB \cos C \sin a).
$$

And similarly,

$$
db = \frac{1}{\sin C}(dB \sin a + dA \cos C \sin b).
$$

Now let the continuous lines represent the orbits of m, m' at certain dates, O, Q the origins of longitude therein; and the



dotted lines the variable orbits of the planets respectively.

Write

$$\text{OC} = \theta,\ \text{CA} = \sigma,\ \angle\,\text{CAC}' = \phi,$$
$$\text{QC} = \theta',\ \text{CB} = \sigma',\ \angle\,\text{CBC} = \phi',$$
$$\angle\,\text{C} = \Phi.$$

Then, answering to the rotation of the lemma, we have

$$a = \sigma',\ b = \sigma,\ \text{C} = \Phi,\qquad d\text{A} = \phi,\ d\text{B} = -\phi'$$
$$\text{or say}\qquad = \tan\phi,\quad = -\tan\phi',$$

whence

$$\text{C}'\text{B} = a + d\,a$$
$$= \sigma' + \frac{1}{\sin\Phi}(\tan\phi\sin\sigma - \tan\phi'\cos\Phi\sin\sigma')$$
$$= \sigma' + \frac{1}{\sin\Phi}(p - p'\cos\Phi),$$
$$\text{C}'\text{A} = b + d\,b$$
$$= \sigma + \frac{1}{\sin\Phi}(-\tan\sigma'\sin\phi' + \tan\phi\cos\Phi\sin\sigma)$$
$$= \sigma - \frac{1}{\sin\Phi}(p' - p\cos\Phi),$$
$$\angle\,\text{C}' = \text{C} + d\text{C} = \Phi - \cos\sigma\tan\phi + \cos\sigma'\tan\phi'$$
$$= \Phi - q + q'.$$

Suppose v, v' are the longitudes of the planets in their two orbits respectively; that is,

$$v = \text{OA} + \text{A}m = \theta + \sigma + \text{A}m,$$
$$v' + \text{QB} + \text{B}m' = \theta + \sigma' + \text{B}m',$$

the predictions of Occultations of Stars by the Moon given in the *Nautical Almanac*, we find the angles at which disappearance and reappearance are computed to take place no longer reckoned from the north round by east, south, and west, but having their initial point at the south, and being measured round by east, north, and west, as in fig. 2.

The reason for this I have utterly failed to discover. It seems to me to introduce a wholly needless complication, and one particularly embarrassing to the student and young astronomer; and I would therefore venture to submit that the mode of reckoning angles in the case of lunar occultations should be assimilated to that employed for measuring the position-angles of double stars. This need not, of course, prevent a supplementary calculation of the angle from the Moon's vertex, if thought necessary or desirable; but even this I would have measured round by the west, and not by the east, so as still to preserve the identity of the two systems.

Forest Lodge, Maresfield, March 2, 1872.

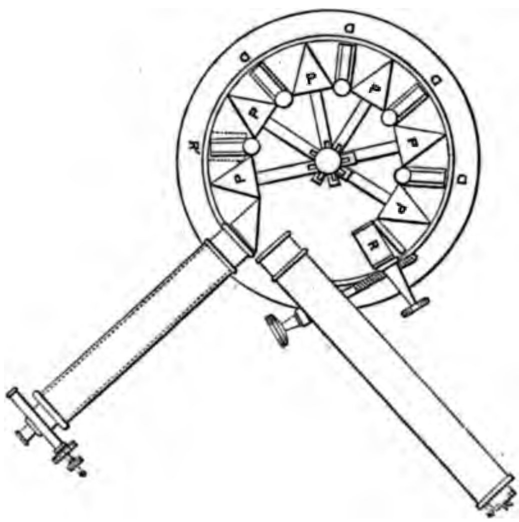
Note on an unsuspected Cause of Diffraction Phenomena in a Telescope. By Capt. William Noble.

Some little time ago, in observing *Jupiter* and his satellites, I remarked certain emanations which appeared to have their origin in diffraction. I was very much puzzled to imagine in what way these phenomena could arise. My 4·2-inch Ross object-glass is simply perfect; my eyepieces were carefully cleaned, and, so far as I could see by removing them, and looking up the tube at the Moon or *Jupiter*, the tube itself was free from any obstruction. Since, however, the general definition of the instrument was sensibly unimpaired, I took no further action in the matter, and let things take their course until about the middle of last month. I was observing the Sun one morning at that period, when I removed the eyepiece for some reason, and happened to glance obliquely up the tube. To my astonishment I saw, brilliantly illuminated by the Sun, a perfect *grating* of excessively fine spider-webs, spun vertically across the interior of the telescope, somewhat within the focus of the object-glass. A light, in more senses than one, suddenly broke in upon me, and I very speedily removed the offending lines. I had the pleasure, that same evening, of viewing the Jovian system shorn of all optical appendages. I am too ignorant of the Arachnida to be able to guess at the species which produces a web of such extraordinary tenuity; but it certainly must be an extremely minute one, not only on account of the excessive fineness of the filaments which it spins, but also in order that it should have found its way inside of a tube so thoroughly and carefully closed as (I should think) to prevent the existence of interstice or aperture whatever whereby an entry might be effected.

Forest Lodge, Maresfield, March 2, 1872.

On an Universal Automatic Spectroscope.
By John Browning.

The diagram represents my automatic spectroscope, so made that any number of prisms from 1 to 6 may be used at pleasure; and as the ray is reversed, the power thus gained will vary from 2 to 12 prisms. This is achieved by so adjusting the right-angled prism that it may be slid into dovetails between each of the prisms, and the ray thus turned back at any part of its course. This can be done with as little trouble as is caused in changing the eye-piece of an ordinary microscope. None of the adjustments of the instrument need be interfered with, and the automatic motion is correct for any number of prisms. A simple contrivance is attached to the micrometer screw, so that it may be thrown out of gear, and the prisms set for any line at pleasure.



With sufficient care the adjustments of the prisms may be made so accurately that any line which falls on the spider-webs of the micrometer eye-piece will appear on the line when any number of the prisms are used. This forms the best method I have yet tried of accurately adjusting a spectroscope of high dispersive power, and enables me to obtain better definitions than I have hitherto done.

In the diagram P P &c. are the ordinary equilateral prisms. R is the reflecting prism shown at the end of the train; in which case, as the ray is reversed, a dispersive power equal to 12 prisms would be obtained. R′ shows the same prism in dotted lines, placed behind the first prism; thus giving the lowest dispersive

power with which the instrument could be used, equal to 2 prisms. D D̊ D represents the dovetails in which the reflecting prism can be made to slide between any of the equilateral prisms.

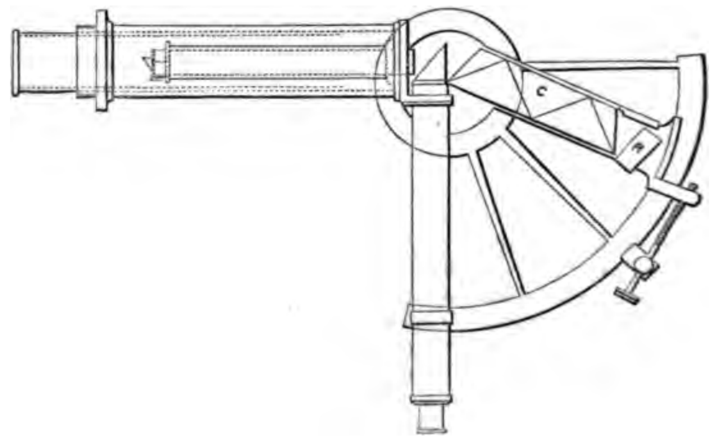
In this instrument both the telescope and collimator remain stationary, so that neither the light nor the eye of the observer need be moved during a whole series of observations. By simply turning the micrometer-screw the train of prisms is moved, so that the whole of the spectrum passes before the eye of the observer, each ray, as it comes to the centre of the field of view, passing through the whole of the prisms at the minimum angle of deviation; thus ensuring the finest definition.

Such a spectroscope, fitted up as a tele-spectroscope, and used with a telescope above 6 in. in aperture, would answer equally well for observing the spectra of the stars, nebulæ, comets, planets, or the Sun, thus saving the multiplication of apparatus, and the great inconvenience of shifting the instruments when they were in adjustment.

While possessing these advantages, this instrument is at the same time more easy to use than any ordinary spectroscope.

On a Tele-Spectroscope for Solar Observations.
By John Browning.

The instrument made by me for Mr. Lockyer appears to answer its purpose very well, yet it leaves something to be desired on the score of lightness, and the manner of attaching



it to the eye-piece of the telescope makes it necessarily expensive. I have therefore sought to introduce an instrument which could be used with greater facility. This I have accomplished by

using a Rutherford's compound prism, as described in my paper, No. 7, vol. xxxi. of the *Monthly Notices.* The ray is reversed twice, so that the power of this prism is quadrupled. As the instrument is made as light as possible, it can be screwed into the eye-piece of a telescope in the same manner as an ordinary eye-piece, and it is sufficiently light to be used with a telescope of 4 in. in aperture.

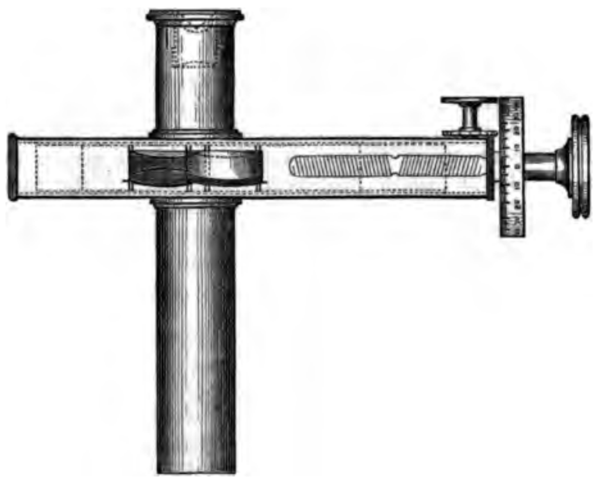
In the diagram C is the compound prism; R the reflecting prism by which the reversion of the ray is accomplished. A second prism of a similar kind is placed directly underneath this, in such a manner that it could not be shown in the diagram.

In using the instrument, it must be so arranged that the image of the Sun falls in focus on the slit.

The dispersive power of the instrument may be varied to suit the aperture of the telescope employed, so as to be equal to between 6 and 12 equilateral prisms of dense flint-glass.

On a Double-Image Micrometer. By John Browning.

As double-image micrometers can be used on a telescope without a clock-driving apparatus, and without the field of view being illuminated, they would doubtless be more employed if there were not important drawbacks to their general use.



Having recently considered the various contrivances used as double-image micrometers, I came to the conclusion that an instrument in many ways more advantageous might be made by simply dividing an ordinary Barlow lens, and causing the two

halves to move separately by means of a micrometer-screw. (See Diagram.) Having had this contrivance made, I find its performance even more satisfactory than I had anticipated. It is compact, inexpensive, and possesses the great advantage that it may be used with any eye-piece.

A Suggestion in the Use of Chronometers, with a view to its use in the approaching Transit of Venus. By David Gill, Jun., F.R.A.S.

Whilst equal increments of heat produce nearly equal effect of retardation on an uncompensated watch, the same increments of heat do not cause the weights of the ordinary chronometer compensation balance to approach the centre of the balance with equal increments.

Besides this, to produce perfect compensation the weights should approach to the centre with an increasing ratio to the temperature, and thus the result is to produce an irrationality of compensation, very analogous to the irrationality of spectra of crown or flint glass, producing secondary spectra in an object-glass.

It is evident from this that if such a balance is adjusted to produce perfect compensation at two very extreme temperatures, it will be over-compensated in mean temperatures.

Many circumstances have been made to correct this, notably by Mr. Hartnup, of Liverpool Observatory, by Messrs. Loseley, Kullberg, and others.

These have been attended with more or less success, but all require very great care in execution and much nicety in adjustment; so that it is still a question if the old form of balance, with certain precautions in its use, is not capable of giving the best results. What I would propose is this, that in the same box with the chronometer should be another chronometer, which we shall call the chronometric thermometer, provided either with an uncompensated brass balance, or a balance where the positions of brass and steel are reversed so as to exaggerate, instead of diminishing, the effects of temperature.

The *true* rate of the chronometer is then to be found for each rate of the chronometric thermometer relative to the chronometer.

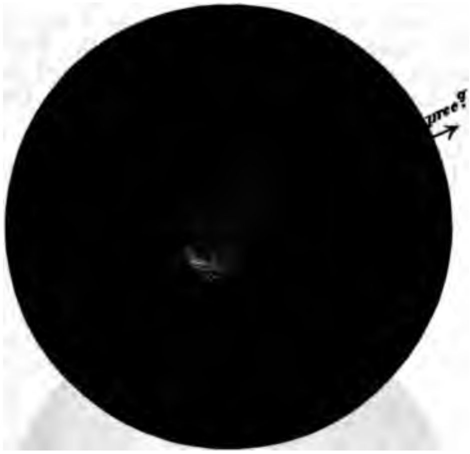
Both Greenwich and Liverpool now possess the most perfect means for ascertaining such rates, and could issue them in a tabulated form, with each instrument so tried.

If this were well carried out, and attention also paid to acceleration of mean rate, and allowance to be made for it, as pointed out by Mr. Hartnup, I am convinced that more perfect results than have been hitherto attained by chronometer transport could be arrived at.

Note on Encke's Comet. By the Rev. Henry Cooper Key.

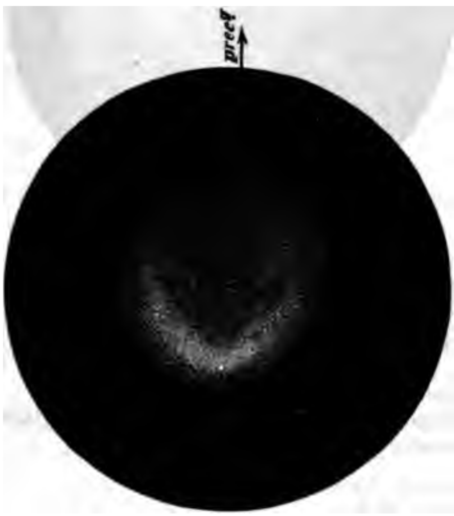
(*Extract from Letter to Dr. Huggins.*)

I send some drawings I made of Encke's comet. The last one taken, on Dec. 3rd, differs slightly from your description as



Encke's Comet. Nov. 5, 1871.

I read it in some periodical. The train following the comet was



Encke's Comet. Nov. 8, 1871.

quite broad in my telescope, and could not be termed a "ray." You will observe two rays on the preceding side; these I have

drawn as you see, but I am *not perfectly certain* that the effect was not in my own eye and not a reality. I took every precaution to find out; and at the time (as well as now) felt pretty well convinced that it was no illusion. Four or five times I left the telescope, and, upon returning, there were the rays in exactly the same spot and direction. I feel pretty confident of their reality (they were extremely faint), but, as I say, am not *quite* certain, as I sometimes see dark lines in the field when first going to the telescope. The comet never seemed to me to lose its elliptical form from the first night I saw it, Oct. 20th. I detected a nucleus for the first time on Nov. 7th. The train I mentioned before was much fainter than the main body of the comet, and I was able to trace it to a distance of about 32′ from the nucleus. I saw nothing like the drawing of the comet made

Encke's Comet. Dec. 3, 1871.

at Greenwich, of which there is a woodcut in the November number of the *Notices*. In this woodcut the engraver, or some one, appears to have made a strange error as to the comet's direction of motion.*

The telescope I used was my 18-inch silvered glass Newtonian equatoreal; powers 92 and 142.

Stretton Rectory, Hereford,
Jan. 1st, 1872.

* Mr. Carpenter remarks that the dotted line through the comet was not intended as an indication of motion, but merely to show the inclination of the comet's axis to the equator.

Russian Preparations for the Observation of the Transit of Venus of 1874. By M. Otto Struve.

(*Extract from a Letter addressed to the Astronomer Royal*).

With regard to the preparation for the transit of *Venus*, I am glad to say that here in Russia all goes on in a regular way.

The inquiries on the meteorological conditions of the selected stations have given, on the whole, very satisfactory results, particularly for the stations on the coast of the Pacific Ocean, and in Eastern Siberia (about 85 per cent clear sky in December). There are only two stations, Taschkent and Astrabad, for which these conditions are not sufficiently satisfactory; therefore the observers designed for Taschkent will probably go to a place about 100 miles west of that town; and instead of Astrabad, we shall either take the island of Aschuradeh, in the Caspian Sea, or, if possible, cross the Elburz mountains and establish our observers at Schahrech in Persia, with nearly an absolute certainty of clear sky.

The total number of Russian stations will be twenty-four, but each of them will be provided with only *one* instrument for the transit-observation itself. We hope in this way to increase the meteorological chances. The instruments will be,—

1. Three 4-inch heliometers, ordered from Repsold.
2. Three photoheliographs, ordered from Dallmeyer.
3. Four 6-inch and four 4-inch equatoreals, ordered from Repsold; all provided with clockwork, filar micrometer, and spectral apparatus.
4. Ten 4-inch telescopes, designed merely for contact-observations.

Of course each station will be also provided with clocks, chronometers, and the instruments required for the exact determination of time. The principal instruments are already ordered, and most of them will be here in the course of the present or beginning of next year. For these instruments, also, the observers are in a great part already nominated. They will all come to Pulkowa for a certain time next year, to exercise themselves in the observations.

The geographical positions of the selected stations will not be determined by the transit-observers themselves; but all stations at which the transit has been successfully observed will afterwards be carefully determined by special expeditions of the General Staff of the Navy. For this purpose a principal line of telegraphic longitudes will be laid (probably next year) through all Siberia to Nicolajensk, with which line the other stations in that part of the empire can be easily joined, either by telegraphic or chronometric operations.

With regard to the photographic method, I can inform you that in two places—at Wilna, under the direction of Colonel

Smysloff, and at Bothkamp in Holstein by Dr. Vogel—they have perfectly succeeded in taking instantaneous photographs of the Sun with *dry* plates.

As far as I know, in Germany the preparations have not much advanced since last spring. The estimates have not yet been laid before the Reichtag, but it is expected that this will be done early in the spring. In the meanwhile Dr. Winnecke has been engaged with practical experiments concerning the heliometric method. He seems to be quite satisfied with the results. Also M. Paschen and Professor Seidel have been working on the photographic method, and Professor Zöllner on the application of the spectroscope for the observation of outer contacts. On these points I am not yet enabled to communicate positive results.

Pulkowa, February 26, 1872.

Eclipse of December last. Australian Expedition. By H. O. Russell, Esq., Government Observer at Sydney.

You are, no doubt, aware that the Royal Society of Victoria made arrangements to observe the recent total eclipse of the Sun. As the Government of New South Wales liberally contributed 300*l.* towards the expense of the expedition, I, with an observing party of five persons, joined the expedition to represent New South Wales. As no vessel offered in Victoria at any sum within the means of the promoters of the expedition, the *Governor Blackall* steamer was fitted out in Sydney for the expedition, and started, on the 27th of November, for Cape Sidmouth. Circumstances induced the observers to select No. 6 Woody Island in preference to the mainland, and upon that part of it (a mere sandbank), which is dry at high water, the two observatories (Victoria and New South Wales) were erected.

As all the instruments sent out by the Royal Society were used by the Victorian party, I was obliged to take such instruments as I could command in Sydney. These were a 7¼-inch refractor, by Merz and Son, mounted equatoreally, and driven by the clock; the eye-piece was removed, and a camera-box put in its place for the purpose of taking photographs during totality, a camera lens of 3-inch aperture and 30 inches focus, mounted in a very light camera, weighing when complete only 6 lbs., was attached to the tube of the refractor, and arrangements were made to uncover the telescope and camera by the same motion of the operator's hand. Two small telescopes, 2¾ and 2¼ inches apertures, mounted equatoreally, and driven by clockwork: in each of these position lines at right angles, were placed to facilitate making the drawings. Also, a 3½-inch telescope, mounted equatoreally, and moved by hand. Lastly, tents and a complete pho-

tographic dark room, with 20 baths, and other things in proportion, to take 20 pictures during totality.

On the day they were landed all my instruments were put up and in part adjusted; during the next day adjustments were completed: and on the third day photographic practice began, and some good pictures were obtained. Experiment also convinced me that we might keep a plate out of the bath at a temperature of 80° for ten minutes, expose it and keep it ten minutes more, before developing, without any serious injury.

On Monday morning we had a full rehearsal and practice all day. All this time, though there had been a continued haziness in the atmosphere, we hoped that we might yet have it clear enough to see something. Monday evening, however, brought heavy clouds and a very severe thunder-storm, during which the lightning passed down the iron stays of the ship five times; and exploded between the stay and the hull, fortunately without damage. Heavy rain followed and continued at intervals all night and the next day; every preparation was, however, made, and nine plates prepared for exposure, even though it rained heavily, and no hope was left but in a possible rift in the clouds. One minute before totality I saw through dense clouds the line crescent of the Sun, but during totality nothing could be seen but a faint light in the clouds in the direction of the Sun; two plates were put in, and exposed 40 seconds, but not a trace of anything was obtained. During totality it was not dark: a peculiar blue grey, extending over clouds and landscape, gave yet abundance of light to read ordinary print. First and last contact were not seen: indeed, except at $1^h\ 40^m$ P.M., and one minute before totality, I saw nothing of the outline of the Sun.

During all the eclipse our sky was entirely overcast, and yet fifteen miles north of us the crew of a small schooner saw the eclipse, their attention being drawn to it by the darkness. It was quite evident from their evidence that there were light clouds over the Sun during totality; and although they state that there was a ring of light at the spot where they had seen the Sun, their description was very unsatisfactory and vague.

The two small telescopes driven by clockwork were taken in accordance with the instructions; larger ones could have been obtained if I had been able to get spectroscopes to use with them, but I could not.

Sydney Observatory, 29th December, 1871.

Note on the Nebula surrounding Eta Argûs. By H. O. Russell, Government Astronomer at the Sydney Observatory.

In Mr. Lassell's "Remarks on the supposed Change in the Great Nebula near η *Argûs*" there is one passage near the foot of p. 253 about which I have a few words of explanation to give.

The passage runs, "It is much to be regretted that Mr. Russell did not accompany his letter by a copy, more or less elaborate, of the drawing and stars he has so carefully laid down," &c.

On the 24th March, 1871, I sent to Sir John Herschel *four copies* of my drawing of the nebula in question, together with measures of the stars and notes of observation; also a statement of the dimensions and quality of the Sydney refractor. *On the 13th of May following* I wrote another letter to Sir John Herschel, enclosing a copy of the report which has got into Mr. Lassell's hands: that letter refers to information previously sent, and I can only regret that the drawings of the nebula there referred to were not put into Mr. Lassell's hands with the newspaper report.

December 29, 1871.

Measures of Binary Star ξ Ursæ Majoris. By George Knott, Esq.

I have obtained during the past two months the following three sets of measures of the interesting binary star ξ *Ursæ Majoris*, now near perihelion:—

P = 31°·60,	obs. 6,	w. 35;	D = 1″·092,	obs. 6.	w. 30;	Epoch 1872·028
29·69,	5,	29	1·111,	4,	24	1872·088
28·15,	5,	36	1·052,	4,	24	1872·138

The measures were taken with my 7⅓-inch Alvan Clark refractor and a parallel wire micrometer; magnifying powers 450 and 515.

Woodcroft Observatory, Cuckfield, Sussex,
March 7, 1872.

Longitude of Teheran. By Lieut.-Col. J. T. Walker.

In September last an approximate value of the longitude of Teheran, which was required for the correction of the maps of Persia, was determined by Col. Walker, R.E., Superintendent of the Trigonometrical Survey of India, and Major St. John, R.E., of the Persian Telegraph Department, through the line of the Indo-

European Telegraph Company. The Directors of that Company had kindly granted the free use of the line for the purpose, and every assistance was most obligingly rendered by the Messrs. Siemens, who supervise all the arrangements for keeping the line in working order.

Signals were sent from London by Col. Walker, and from Teheran by Major St. John, the Greenwich times of the signals being ascertained from a clock in the Government Central Telegraph Office, which was governed by a clock in the Greenwich Observatory, while the Teheran times were determined by sextant observations taken on the spot by Major St. John, and his assistant, Capt. Pierson, R.E.

Considerable interest attaches to the operations from the circumstance that though the distance from London to Teheran, along the telegraph line, is nearly 4000 miles,* and it was necessary to employ automatic relays at five intermediate stations, the entire retardation of the electric current in either direction was found to average less than half a second. This shows that the line is in a very high state of efficiency, which is most creditable to the Messrs. Siemens, by whom all the working arrangements are carried out. Thus there is much reason to hope that, when the necessary instruments are available in India, exact and final determinations of the differences of longitude of the Greenwich and the Madras Observatories, and the stations on the arcs of parallel of the Indian Survey, may be obtained without any serious difficulty.

The value now determined for the longitude of Teheran is 51° 24′ 56″ east of Greenwich. It differs by less than half a minute, or say half a mile, from the value which had been previously deduced by Major St. John by combining a telegraphic determination of the difference between Teheran and Kurrachee, which was made by himself and his assistants, with the trigonometrical difference between Kurrachee and the Madras Observatory, which is furnished by the operations of the great Trigonometrical Survey of India, and assuming for the Madras Observatory the latest and most exact value of longitude, 80° 14′ 20″ east of Greenwich, which has been adopted by the Government astronomer at Madras, and is quoted in all the recent nautical almanacs. This close coincidence between two independent results, though possibly to some extent fortuitous, may be accepted as

* Linden to Emden	390 miles.
Emden to Berlin	330
Berlin to Gitomis	850
Gitomis to Kertch	800
Kertch to Tiflis	700
Tiflis to Teheran	800
Total	3870 miles.

a sufficient proof that there can be no very material error in the adopted value for the Madras Observatory, and this is a matter of some importance, as all the most important determinations of longitude in India have invariably been referred differentially to that observatory.

Note on Colour as affected by Variation of Optical Power. By Lt.-Col. A. Strange, F.R.S.

At one of the meetings of the Society last year, in a discussion on the changes of colour that had been observed at distant intervals in some of the planets, I stated my doubt whether the same observer at different epochs would be able to compare his impressions of colour satisfactorily. My belief is that of the three elements, form—size—colour, the last is that which is least permanently fixed, in most cases, in the sensorial memory.

A recent, perfectly unpremeditated, experiment bears upon the general question of estimating or distinguishing colour, though not upon the above particular branch of it. I was, within the last few days, at a theatre with two young ladies. They drew my attention to "the lady opposite in *pink*." Turning my glass to her, I replied, "You mean the lady in *yellow*." "No," replied both, "her dress is pink." Having ascertained that we all spoke of the same person, I begged my companions to use their glasses. On doing so they both at once admitted the colour to be yellow, as I had said. But they assured me that to their naked eye it was pink as before.

One of the young ladies, my own daughter, is considered to have a remarkably fine eye for colour, with the faculty of matching and remembering tints very strongly developed. The other, also a near relative, is likewise an excellent judge of colour, and a born artist. The dress about which the above doubt arose was not all coloured. It was white, with a great deal of the doubtfully-coloured trimming, and the tint (whatever it really may have been) was *very pale*. There was strong light upon it, and the distance was that of the whole greatest diameter of a small theatre.

Such are the facts, on which I do not propose to theorise. But they certainly point at least to one practical consideration—namely, the influence of optical power on colour impressions, and the necessity of great caution in pressing observations on the colour of heavenly bodies into the service of speculations regarding cosmical changes.

Since the above was written I have read Mr. Lassell's interesting paper on the planet *Jupiter* (*Monthly Notices* for January 1872), in which is the following passage,—"I may add further, that the variety of colour is most obvious with the higher powers, and when the atmosphere will not permit their use the tints are scarcely apparent."

Summary of Sun-spot Observations made at the Kew Observatory during 1871. Communicated by Messrs. De La Rue, Stewart, and Loewy.

Months.	Days of Observation.	Numbers given to the new Groups in the Kew Catalogue.	Number of New Groups.	Days without Sun-spots.
January	13	No. 1529 to No. 1547	19	0
February	16	1548 ,, 1576	29	0
March	26	1577 ,, 1619	43	0
April	21	1620 ,, 1647	28	0
May	25	1648 ,, 1677	30	0
June	21	1678 ,, 1698	21	0
July	21	1699 ,, 1719	21	0
August	23	1720 ,, 1740	21	0
September	19	1741 ,, 1752	12	c
October	14	1753 ,, 1765	13	0
November	13	1766 ,, 1786	21	0
December	7	1787 ,, 1799	13	0
Total	219	No. 1529 to No. 1799	271	0

A comparison of the numerical results with those published in the *Monthly Notices* of the Royal Astronomical Society for the year 1870, will prove that the latter year includes the maximum of Solar activity during the present period. The diminution is not only manifested in the numbers of the new groups, but also in their magnitude. Indeed, there is so marked an absence of really large and striking groups during the past year, that it more fully bears out our opinion, expressed on several previous occasions, that only by a continuous and well-devised plan of determining the absolute magnitudes of the Sun-spots from day to day, and from year to year, will light be ultimately thrown upon the true nature and relation of the forces, which regulate a series of phenomena of so well-established a cosmical interdependence that their discovery will undoubtedly mark a greater epoch in astronomical science than that of the universality of gravitation.

A striking feature in last year's observations seems to have been the occurrence of groups in comparatively high latitudes, especially in the Southern Solar hemisphere; a group observed between the 21st and 23rd of March had the almost unprecedented high latitude of $-43°$, while latterly, towards the end of the year, several groups in almost as high a latitude have repeatedly made their appearance. Such Polar groups appear to be the more short lived, the greater the distance from the well-defined equatorial region of common Solar activity; and there are indications that they are produced by sudden, but most violent, convulsions: the group of March, above alluded to, left long after its disappearance a ridge of faculous matter behind it, covering an area of at least twenty times the magnitude of the spot itself, and spreading out along a parallel of latitude more in the manner of a

cometary appendage than one of the usual faculæ which surround the central area of depression.

The Solar Prominences. By Father Secchi.

(*From the Comptes Rendus.* Abstract.)

"I have the honour of presenting to the Academy the result of all my observations made on the Prominences during the year 1871. I think it indispensable to present this summary, for it demonstrates that the conclusions, announced after the first observations, are confirmed by a sufficiently long period, comprising nine solar rotations. New facts also are made manifest, which bear importantly on the solar theory. The nine synodic rotations (approximate), and the number of days of effective observations, were distributed as follows :—

Rotation.		Days of observation.
I.	April 23rd to May 21st	25
II.	May 22nd to June 18th	24
III.	June 19th to July 15th	26
IV.	July 16th to August 12th	28
V.	August 13th to September 9th	25
VI.	September 10th to October 7th	18
VII.	October 8th to November 9th	14
VIII.	November 5th to December 4th	8
IX.	December 5th to December 31st	16

"The total number of prominences noted and figured amounts to 2667; the total number of days of complete observation is 184.

"The following table* contains the number of observed prominences arranged in bands of heliographic latitude ten degrees wide :—

	Number of Prominences.	
	Northern Hemisphere.	Southern Hemisphere.
90° to 80°	118	139
80 ,, 70	142	180
70 ,, 60	73	87
60 ,, 50	87	61
50 ,, 40	125	156
40 ,, 30	182	183

* In the original paper the numbers are given in all the tables for each rotation. The tables so arranged would not be suited to these pages. Moreover, none of the results dwelt on by Secchi as depending on latitude, require the evidence of individual rotations. He considers the results for the whole Sun, with reference to the different rotations; and the necessary evidence for this purpose is here presented separately.

	Number of Prominences. Northern Hemisphere.	Southern Hemisphere.
30 „ 20	203	223
20 „ 10	195	228
10 „ 0	183	202
Sums .	1208 N.	1459 S.
Total	2667	
Mean number per day		14·49

"We see that there are two principal maxima of frequency placed between 20 and 30 degrees of north latitude, and between 10 and 30 degrees of south latitude; two secondary maxima are found between 70 and 80 degrees in each hemisphere. The principal minima fall between 60 and 70 degrees north and 50 and 60 degrees south; one secondary minimum near the equator, between zero and 10 degrees north; and two other minima at the poles. In successive rotations, there is no trace of a progressive motion of the principal maximum towards the poles as had been suspected; if there is any small oscillation or displacement the phenomenon lasts but a short time; we shall see in future times whether this will be confirmed. The mean numbers of prominences per day of observation were as follows, for the rotations numbered I. to IX. above :—14·24; 16·12; 14·85; 15·78; 14·96; 14·61; 14·28; 13·75; 15·56; showing that the number of prominences reached a maximum between May and June, and a minimum between September and November. These variations correspond with the solar activity as deduced from the frequency of spots.

"But, to judge of the solar activity, the number of prominences is not enough; we must consider their height. For this purpose the following table has been prepared. It shows the mean height of all the observed prominences, this height being expressed in terms of an arbitrary unit, convenient for the observations, and corresponding to eight seconds (for convenience in drawing):—

	Mean Height of Prominences. Northern.	Southern.
90° to 80°	5·08	5·45
80 „ 70	5·28	5·77
70 „ 60	4·27	4·73
60 „ 50	5·25	4·69
50 „ 40	5·81	5·93
40 „ 30	6·69	7·03
30 „ 20	6·32	6·92
20 „ 10	5·62	6·07
10 „ 0	5·90	6·00
Mean .	5·66	5·90
Absolute mean . . .	5·78	

"This table proves that the regions where the prominences are most numerous are those also where they are most lofty. The mean height diminishes from the second to the eighth rotation, or from May 22nd to December 4th, when it was reduced in the proportion of 4 to 7. (After this rotation it appeared to increase again.*) In the southern hemisphere the prominences are somewhat higher, as well as more numerous, than in the northern.

"The better to evaluate this interesting element, the following table has been prepared. It shows the number of prominences having a height exceeding 40 seconds, and the number exceeding 60 seconds in height:—

	Number of Prominences 40″ high and upwards.		Number of Prominences 60″ high and upwards.	
	Northern.	Southern.	Northern.	Southern.
90° to 80°	77	92	18	23
80 " 70	110	148	33	43
70 " 60	41	53	14	19
60 " 50	52	33	26	7
50 " 40	77	115	36	39
40 " 30	134	140	65	67
30 " 20	154	169	65	71
20 " 10	132	175	40	69
10 " 0	138	130	44	61
Sums	925	1039	343	408
Total	1964		751	
Mean number per day	10·67		3·22	

"It follows that the positions of the maxima and minima are nearly identical with those shown in the general table; and the secondary maximum, near the polar zone, remains very well marked. The total numbers observed during the rotations were as follows:—

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Prominences above 40″	211	334	342	360	297	150	96	54	120
Prominences above 60″	100	142	164	132	107	34	25	10	37

"We see that after the fifth rotation there is a marked falling off in prominences of both orders.† This particular state of things corresponds visibly with the number of spots, which diminished greatly after September 10. The solar diameters also, as determined by the chronograph, showed greater regularity during this period of solar tranquillity.

* Or rather, perhaps, the diminution was too irregular to be properly indicated in periods of single rotation, as appears from the following numbers, giving the mean height for the nine rotations:—6·26, 7·38, 7·12, 6·25, 6·17, 5·04, 4·96, 4·05, 4·81.

† It will be remembered that the first order includes the second.

"In the following table the extent of the prominences in solar latitude is indicated, as well as the frequency of faculæ :—

			Mean width of Prominences.		Number of Faculæ.	
			Northern.	Southern.	Northern.	Southern.
90°	to	80°	5·02	5·12	61	61
80	"	70	5·82	5·56	75	94
70	"	60	4·49	5·05	83	106
60	"	50	5·10	5·51	95	100
50	"	40	5·68	5·37	150	142
40	"	30	5·99	6·01	223	231
30	"	20	6·12	5·83	295	317
20	"	10	6·06	6·03	280	315
10	"	0	5·81	5·66	170	192
		Mean	5·57	5·58	Sums 1432	1558
Absolute mean			5·575		Total 2990	

"We see that the regions which furnish the maximum numbers* are those also in which the prominences have their greatest extension. In successive rotations, diminished height corresponds to diminished width, as appears from the following mean widths for the nine successive rotations :—6·22, 5·65, 6·52, 5·48, 5·45, 4·75, 4·93, 4·87, 6·30.†

"The parallelism between height and width cannot be pursued further, for often a great extent of chromosphere is very high and very bright; but it does not reach higher than 24 seconds, the limit conventionally assigned to characterise a prominence. Be this as it may, however, we find that the prominence-masses which are highest are also those which have the greatest extent in longitude and latitude, though we meet sometimes with narrow and isolated prominences reaching to a great height.

"The classification of the positions of faculæ shows (1) that the regions richest in faculæ coincide with the regions already indicated where the prominences have their maximum height and width; (2) that the faculæ have a secondary minimum at the equator; and that beyond the maxima placed between 10 and 30 degrees, they decrease regularly up to the poles; but yet (3) that near the poles there are regions where rather bright granulations reach near to the border, and counting these we should find a secondary maximum of faculæ on the borders of the polar zones; but the limits are very difficult to observe, and have not always been taken into account.

* The text has been departed from here Secchi writes:—" Les régions qui fournissent des maxima *et des minima* pour le nombre des protubérances sont aussi celles dans lesquelles les protubérances sont le plus étendues." The italicised words should obviously be omitted.

† Secchi does not sum up the number of faculæ in the different rotations, but their agreement with the prominences in this respect is close and significant, the faculæ being always most numerous when the prominences are so, and *vice versâ*.

"Among the 893 prominences observed between August 26th and December 31st, 471 were found to have a well-marked slope, resembling inclined plumes; of these, 370 were inclined in agreement with the law of movement of the solar atmosphere, from the equator towards the poles, and only 101 were sloped in the contrary direction; 40 were vertical, at the poles and equator. Comparing this result with that mentioned in former communications, we see that it cannot be accidental. I see that M. Spörer has arrived at the same result, later. I may remark here that during the epochs of greatest activity the law is more constant and decided."

Observations of the Planet (117) *Lomia, made at the Marseilles Observatory.* By MM. Stéphan and Borrelly.

Date of the Obser. 1871.	Hour of the Observation Marseilles Mean Time. d h m s	Apparent R.A. of (117) h m s	Log. f. Par.	Apparent Decl. (117) ° ′ ″	Log. f. Par.	Observer.	Comparison Star.
Sept.	12 10 47 12	23 49 6·25	+1·2477	+1 18 16·5	0·7759	B.	*a*
	12 12 46 28	23 49 1·57	2·3885	1 18 11·1	0·7722	S.	*a*
	13 9 45 41	23 48 14·47	1·4301	1 17 34·9	0·7772	B.	*a*
	15 9 0 39	23 46 27·08	$\bar{1}$·5025	1 15 49·8	0·7783	B.	*f*
	15 11 10 12	23 46 21·88	$\bar{1}$·0367	1 15 43·2	0·7753	S.	*b*
	16 8 18 47	23 45 33·44	1·5582	1 14 53·6	0·7800	B.	*a*
	18 10 11 1	23 43 36·39	1·2762	1 13 54·6	0·7763	S.	*b*
	19 12 42 37	23 42 37·95	$\bar{2}$·9981	1 11 46·4	0·7762	S.	*c*
	22 13 23 54	23 39 51·41	1·3027	1 8 38·7	0·7773	S.	*c*
	29 21 36 32	23 33 41·13	2·8327	1 1 23·9	0·7775	S.	*d*
	30 12 45 43	23 32 47·21	1·3017	1 0 18·2	0·7783	S.	*d*
Oct.	15 12 12 57	23 21 35·54	1·4107	+0 48 27·9	0·7805	S.	*e*

Adopted Mean Positions of the Comparison Stars, for 1871·0.

	Names of the Comparison Stars.	R.A. h m s	Decl. ° ′ ″	Magnitude.
a	25 Poissons 8303 B.A.C.	23 46 28·26	+1 22 26·7	6·5
b	869 Weisse, hora 23	23 43 31·95	+1 15 14·1	9·0
c	4779 Arg. Z. + 1°	23 43 22·10	+1 10 49·8	9·3
d	8243 B.A.C.	23 35 28·27	+1 4 20·0	5·0
e	5007 Arg. Z. + 0°	23 23 5·68	+0 54 56·8	9·4
f	874 Weisse, hora 23	23 43 45·43	+1 12 7·3	9·0

Nebulæ discovered and observed at the Marseilles Observatory. By M. E. Stéphan.

Mean Positions for 1870·0.

Names of the Comparison Stars.		R.A.	N.P.D.	
		h m s	° ′ ″	
281 Weisse (n.c) H. I.	9	1 12 3·05	74 21 25·9	Very very faint, very small, round, condensed at centre, but no bright point. (Scarcely perceptible.)
583 Weisse (n.c.) H. I.	8–9	1 29 18·33	48 20 28·8	Very very faint, very small, round, brighter at centre, but no bright point, properly so called.
1268 Weisse (n.c.) H. I.	8	1 54 53·40	58 33 30·8	Two neighbouring nebulæ. The two nebulæ are very very faint, very small; no bright points. The first is slightly more extended than the second, which is scarcely perceptible.
		1 55 46·82	58 33 7·3	
3971 Lalande	8½	1 58 29·25	77 22 17·8	Round, very small, very faint, more condensed at the centre, but no bright point, properly so called.
4713 Lalande	8½	2 23 28·11	59 49 52·1	Round, very small, not bright, bright points near the centre, seems resolvable.
465 Weisse (n.c.) H. II.	6–7	2 23 31·59	60 59 25·0	Very very small and faint, round, brighter at centre.
502 Weisse (n.c.) H. II	9	2 25 3·84	62 30 24·1	Extremely faint, small, gradually brighter towards the middle. Bright point.
655 Weisse (n.c.) H. II.	8	2 26 35·20	61 15 40·9	Moderately bright, of irregular form, lengthened in a direction inclined about 75° to the meridian; several bright points.
631 Rümker H. II.	6	2 27 18·08	67 9 31·1	Round, very very small, very faint, bright centre.
786 B.A.C.	6	2 27 19·20	59 3 6·4	Round, very very faint, very small, condensation at centre.
665 Rümker	9	2 30 17·32	49 2 13·8	Group of seven neighbouring nebulæ; all are extremely small and faint. No. 3 almost imperceptible; 1, 2, 5, and 6 have nearly the same brightness; 4 is rather diffuse, and somewhat more extended than 1, 2, 5, and 6, but fainter; 7 is less faint than the rest, a bright point in the middle.
,,		2 30 24·64	48 55 7·0	
,,		2 30 32·33	48 53 45·6	
,,		2 30 35·46	49 6 22·7	
,,		2 30 57·00	48 53 36·5	
,,		2 31 12·61	49 4 18·5	
677 Arg. Z. + 40°	7·1	2 33 55·63	49 3 20·3	
502 Arg. Z. + 34°	9·4	2 34 39·58	55 47 40·5	Extremely small and faint; a bright point.
1459 Weisse (n.c.) H. II.	9	3 4 9·68	51 10 24·7	Extremely faint, very small, round, no bright point, somewhat diffuse.

Mean Positions adopted of the Comparison Stars, for the Epoch 1870·0.

Names of the Comparison Stars.		R.A. h m s	N.P.D. ° ′ ″
281 Weisse (n.c.) H. I.	9	1 14 57·93	74 17 51·4
583 Weisse (n.c.) H. I.	8–9	1 27 38·10	48 16 14·6
1268 Weisse (n.c.) H. I.	8	1 54 12·99	58 30 37·0
3971 Lalande	8½	2 2 46·50	77 26 25·5
4713 Lalande	8½	2 27 53·72	55 52 50·1
465 Weisse (n.c.) H. II.	6·7	2 20 32·93	60 54 44·9
502 Weisse (n.c.) H. II.	9	2 22 0·27	62 32 22·4
655 Weisse (n.c.) H. II.	8	2 28 3·71	61 20 54·4
631 Rümker H. II.	6	2 21 49·24	67 6 46·9
786 B.A.C.*	6	2 24 11·23	58 59 27·6
665 Rümker H. II.	9	2 29 14·28	48 57 27·8
677 Arg. Z. + 40°	7·1	2 35 26·70	49 3 23·4
502 Arg. Z. + 34°	9·4	2 35 46·86	55 45 47·9
1459 Weisse (n.c.) H. II.	9	3 1 58·38	51 10 29·8

Erratum by Mr. Hunt.

The wood-engraver has made an important omission in the copy of the little map of Struve's stars, at page 91 of the *Monthly Notice* for January. He has omitted the star of Weisse's Bessel Hora xix, No. 745 of the 8th mag., at the centre of the trifolium where the three dotted lines meet. As the map is now drawn it represents simply a triangle, and requires the central star to answer to Sir W. Herschel's graphic description :—" A telescopic trifolium north following K, similar to that on the hand of *Aquarius*." Another omission was made in the map (not, however, important to the subject under discussion), namely, that of Weisse's Bessel, No. 692, 8·5 mag., and R.A., $19^h\ 26^m\ 11^s$, Dec.—10° 44′ 32″.

Chad Road, Edgbaston,
Feb. 5th, 1872.

* Another star must be intended. The place of 786 B.A.C. (15 *Trianguli*) for 1870·0 is in R.A. $2^h\ 17^m\ 52^s{\cdot}66$ and N.P.D. 55° 52′ 50″·88.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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PROFESSOR CAYLEY, President, in the Chair.

Henry Pratt, Esq., 18 Preston Street, Brighton;
H. C. Levander, Esq., 376 Wandsworth Road; and
Geo. Mathus Whipple, Esq., Observatory, Kew,

were balloted for and duly elected Fellows of the Society.

The Second Part of a Memoir on the Development of the Disturbing Function in the Lunar and Planetary Theories. By Prof. Cayley. (Abstract.)

The present communication is a sequel to my paper, "The First Part of a Memoir on the Development of the Disturbing Function in the Lunar and Planetary Theories," *Mem. R. A. S.*, vol. xxviii. (1859), pp. 187–215, and I have therefore entitled it as above, but it in fact relates only to the Planetary Theory. In the First Part, I gave in effect, but not explicitly, an expression for the general co-efficient D (j, j') in terms of the co-efficients of the multiple cosines of θ in the expansions of the several powers $(r^2 + r'^2 - 2\,r\,r' \cos\theta)^{-s-\frac{1}{2}}$, or say $(a^2 + a'^2 - 2\,a\,a' \cos\theta)^{-s-\frac{1}{2}}$; viz., at the foot of page 208 I speak of the term involving $\cos(j\,\mathrm{U} + j'\,\mathrm{U}')$ as having a certain given value; the term in question is $\mathrm{D}\,(j,j') \cos(j\,\mathrm{U} + j'\,\mathrm{U}')$; and consequently the expression for D (j,j') is

$$\mathrm{D}\,(j,j') = \Sigma\,\frac{\Pi_1\,(s-\frac{1}{2})}{\Pi\,x}\,n^{2x}\,\Sigma\,\mathrm{M}_x^{\vartheta}\,\mathrm{R}_x^{\vartheta}\ ;$$

the omission was however a material one, inasmuch as this expression for the general co-efficient serves to connect my formulæ with Leverrier's development, *Annales de l'Obser. de Paris*, t. I. (1855) pp. 275–330 and 358–383, and I resume the question for the purpose of supplying it.

On the Proposition 38 *of the Third Book of Newton's Principia.* By I. Todhunter.

In this Proposition Newton undertakes to determine the figure of the Moon. The Moon is supposed to be a homogeneous fluid, drawn into an oblong shape by the disturbing action of the Earth; the rotation of the Moon on its axis, and the revolution of the Earth with the Moon round the Sun, are disregarded. The Proposition is connected by Newton with one that precedes, in which the Earth is supposed to be fluid, and to be similarly acted on by the Moon.

Let M denote the mass of the Earth, and m the mass of the Moon. Suppose that the Earth is a homogeneous fluid, and that, owing to the disturbing action of the Moon, it assumes the form of an ellipsoid of revolution round the major axis. Let B denote the minor axis, and B + H the major axis, where H is supposed small compared with B; let k denote the distance between the centres of the Earth and of the Moon. Then we shall have approximately

$$\mathrm{H} = \frac{15}{4} \cdot \frac{m}{\mathrm{M}} \cdot \frac{\mathrm{B}^4}{k^3}$$

Newton does not explicitly give this formula; but his numerical results in Propositions 36 and 37 appear to have been deduced from it; and it can be obtained readily in the same way as Newton employed elsewhere: see Whewell *on the Free Motion of Points*, third edition, page 202. The formula was obtained by D. Bernoulli in his prize essay on the Tides, Chapter IV., Article 8.

In like manner let b denote the minor axis, and $b + h$ the major axis of the ellipsoid of revolution, which the Moon becomes when disturbed by the Earth; then

$$h = \frac{15}{4} \cdot \frac{\mathrm{M}}{m} \cdot \frac{b^4}{k^3}$$

Hence by division we obtain

$$\frac{h}{\mathrm{H}} = \left(\frac{\mathrm{M}}{m}\right)^2 \left(\frac{b}{\mathrm{B}}\right)^4 \qquad (1)$$

Newton, however, in his Proposition 38, instead of (1), makes

a statement in words which is equivalent to the following formula :—

$$\frac{h}{H} = \frac{M}{m} \cdot \frac{b}{B} \qquad (2)$$

Newton himself gives no reason for the statement.

In the *Astronomiæ Elementa*, by David Gregory, published at Oxford in 1702, a proposition is ennunciated, which is substantially equivalent to the formula (2), and is supported by a folio page of general reasoning, which is, however, altogether unsatisfactory ; see page 387 of that work.

In the Jesuits' edition of the *Principia* a note is given, which is founded on the process of David Gregory, and is also quite inadmissible.

Newton's error was pointed out with some emphasis by Paul Frisi ; see pages 174 and 186 of his *De Gravitate universali*, 1768, page 135 of the second volume of his *Cosmographia*, 1775, and page 170 of the third volume of his *Opera*, 1785.

If the error had been generally admitted, and the correction adopted, it would of course have been superfluous to recur to the matter ; but so far as I have been able to find, no attention has been given to it. Writers and commentators have sometimes touched on Propositions 36 and 37 in such a manner that we might naturally have expected an allusion to Proposition 38, if only by way of warning ; but nothing is said about it.

For a more decisive case, we may refer to *Robison's Mechanical Philosophy*, 1804. On page 512, he speaks of Frisi's *Cosmographia* as a very masterly performance on the subject of the figure of the planets ; and in his page 517, he alludes to Newton's Proposition III. 38, without any remark as to the criticism on it by Frisi.

But the most remarkable case is that furnished by the book of Dr. Whewell's, which I have already cited. On page 206, Newton's statement is reproduced without any indication whatever of its untenable character. This seems very strange, when we reflect that Dr. Whewell paid great attention to Newton's work, and also to the Theory of the Tides, with which the Proposition has some connexion.

Lord Brougham touches on the subject in his *Analytical View of Sir Isaac Newton's Principia;* he says on his page 295, "The tide in the Moon caused by our Earth is to the tide in our sea caused by the Earth, as the mass of the Earth to the mass of the Moon." I presume that on the second occasion where the word *Earth* occurs, it is a misprint for *Moon ;* and then the statement amounts to the formula,

$$\frac{h}{H} = \frac{M}{m}$$

This agrees neither with (1) nor with (2), and is of course quite wrong.

Laplace has considered the problem of the figure of the Moon in his *Théorie du Mouvement et de la figure elliptique des Planètes.* He takes into account the rotation of the Moon on its axis; but otherwise his solution is consistent with Frisi's result, and not with Newton's; see page 115 of the work. He does not mention either Newton or Frisi.

The Proposition to which this note relates is of no practical importance; but even in pure theory it is well to be accurate and consistent: I have therefore thought it would be useful to draw attention to a curious error which has indeed been corrected, but certainly not obliterated.

The Source of the Solar Heat. By Maxwell Hall, B.A.

Let us suppose that the mass of the Sun is slowly, but continually, contracting; then, in consequence of the enormous mass subjected to this contraction, an enormous amount of heat will be developed, and it will be found that the rate or amount of contraction necessary to produce the amount of heat radiated by the Sun into space, is so remarkably small, that ages must elapse before the effect of this contraction can become visible to us at our comparatively great distance from the Sun.

In order to show that this is the case, let one foot and one second be taken as the units of space and time, and suppose that each unit of volume of the Sun's mass contracts by the same amount in the same time, so that if z_0 be the linear contraction of the Sun's radius r_0 in one second, and if z be the contraction of any other length r, measured from the centre, and for the same duration of time, then $\frac{z}{z_0} = \frac{r}{r_0}$. The effect of this contraction may thus be compared to a series of intermittent pulsations, acting throughout the whole of the mass, and tending to diminish the volume. Let g_0 be the force of gravity at the surface of the Sun, and let g be the force of gravity at any point within the Sun's mass considered homogeneous, whose distance is r from the centre; then $\frac{g}{g_0} = \frac{r}{r_0}$. Again, let ϱ be the mean density of the Sun's mass, so that the weight of any thin concentric shell, whose radius is r and thickness δr, will be $4\,\pi g \varrho r^2 \delta r$; and, since every unit of mass in this shell falls through z feet towards the centre in a second of time, $4\,\pi g \varrho z r^2 \delta r$ will be the kinetic energy generated and destroyed every second by this shell alone; and therefore $\int_0^{r_0} 4\,\pi g z r^2 d r$ will be the whole kinetic energy destroyed every second of time, and we proceed to find the corresponding amount of heat evolved.

Now $\int_0^{r_0} 4\pi g \varrho z r^2 dr = \frac{4\pi g_0 \varrho z_0}{r_0^2} \int_0^{r_0} r^4 dr = \frac{4}{5}\pi g_0 \varrho z_0 r_0^3$,
and this is the kinetic energy destroyed by the fall of a weight $\frac{4}{5}\pi g_0 \varrho r_0^3$ through a height of z_0 feet, or by the fall of a weight $\frac{4\pi g_0 \varrho z_0 r_0^3}{5 \times 1390}$ through a height of 1390 feet; but the fall of one pound avoirdupois through a height of 1390 feet generates sufficient heat to raise one pound of water through 1° centigrade, or it generates one thermal unit; hence by expressing $\frac{4\pi g_0 \varrho z_0 r_0^3}{5 \times 1390}$ in foot-pounds we shall get the number of thermal units generated every second of time. Now $g_0 \varrho$ is the weight of a cubic foot of the Sun's mass at the surface, and since

g_0 = 27·20 times the force of terrestrial gravity, and

ϱ = 1·43 times the density of water, therefore

$g_0 \varrho$ = 27·20 × 1·43 times the weight of a cubic foot of water at the surface of the Earth. But a cubic foot of water weighs 62·5 pounds, so that $g_0 \varrho$ = 2431 pounds.

Therefore $\frac{4\pi z_0 r_0^3 \times 2431}{5 \times 1390}$, when both r_0 and z_0 are expressed in feet, will give us the number of thermal units generated every second of time.

Now, it has been found by observation that the heat emitted by the Sun to the Earth is sufficient to melt a sheet of ice whose thickness is 0·01093 inches, when exposed perpendicularly to the Solar rays for one minute (Sir John Herschel, *Meteorology*), due allowance having been made for the heat absorbed by the atmosphere; and, therefore, in one second, a sheet of ice whose thickness is 0·0000152 feet will thus be melted; so that, if a be the mean distance of the Earth from the Sun, expressed in feet, a spherical shell of ice, whose radius is a and thickness 0·0000152 feet, will be the volume of ice in cubic feet melted every second by the whole of the radiant Solar heat. But one pound of ice has a volume equal to $(0{\cdot}2584)^3$ cubic feet, therefore $\frac{4\pi a^2 \times 0{\cdot}0000152}{(0{\cdot}2584)^3}$ will be the weight of the ice in pounds thus melted.

Again, in order to melt one pound of ice, 79·25 thermal units are required; and thus the whole Solar heat evolved in one second of time is equal to $\frac{4\pi a^2 \times 79{\cdot}25 \times 0{\cdot}0000152}{(0{\cdot}2584)^3}$ thermal units.

Now, by equating the heat generated to the heat evolved in one second, we get

$$\frac{4\pi z_0 r_0^3 \times 2431}{5 \times 1390} = \frac{4\pi a^2 \times 79{\cdot}29 \times 0{\cdot}0000152}{(0{\cdot}2584)^3},$$

and the contraction z_0 is therefore only 0·000004079 feet in one second, or 129 feet per annum; and, as we have already said, ages must elapse before the effect of this contraction can become

visible to us, whether we compare direct measures of the Solar disk or observed periods of axial rotation.

The contraction, therefore, is so small, that as much allowance can be made for the assumptions introduced above as may be thought necessary, without altering our general conclusion in the slightest degree, namely, that the source of the Solar heat and light is connected with the mechanical theory of heat by means of the contraction of the composing mass.

The application of this theory to other bodies is almost without limit; the Earth has contracted, and has stored up a corresponding amount of heat in the non-conducting rocks and soils; the stars, by their intrinsic brilliancy, indicate the operation of the force of gravity upon contracting matter; the nebulæ afford examples of the commencement of this operation; and periodical variations in light now become perturbations, the effect of disturbing masses in motion, producing endless changes subject to the great principle known as the conservation of energy.

January 1872.

On the Insufficiency of Existing National Observatories. By Lieut.-Col. A. Strange, F.R.S.

The Astronomer Royal, in a recent paper* on the "Proposed devotion of an Observatory to observation of the phenomena of Jupiter's Satellites," introduced his remarks in these words:—
"The position which the Royal Astronomical Society holds in the astronomical world may well justify it in employing its judgment and its influence in the direction of astronomical enterprise exterior to its own body."

Adopting Mr. Airy's plea, I now propose to invite the attention of the Society to some further consideration of the question he has raised, as to the sufficiency of existing National Observatories.

These establishments were organised at a time when the objects of astronomical research were comparatively few, readily enumerated, and easily defined. The Royal Observatory was founded in the interests of navigation, and though that purpose has been very liberally interpreted, and much of the work done has had far higher aims, still, on the whole, it has been, for the most part, of a routine, observational character.

But within a comparatively recent period, the science of astronomy has made enormous advances in directions not contemplated when Greenwich was founded. And it has been for some time clear, that Greenwich cannot, as at present constituted, be expected to contribute systematically to the advancement of the new branch of astronomy to which I allude, namely, what

* *Monthly Notices*, Jan. 12th, 1872, vol. xxxiii. No. 3.

now generally recognised as the Physics of Astronomy. I have spoken of Greenwich as the type, and the highest type, of National Observatories; but if we turn to the Report of the Council of the present year, which gives us the latest account of the operations of all such observatories as can be considered in any sense official, we shall find the same characteristic prevailing, namely, a nearly total exclusion of physical work.

I do not state this in order to reflect disparagingly on the proceedings in question. With respect to the Royal Observatory of Greenwich, indeed, there can be no doubt that its strict adherence for so long a period to a well-marked system of almost purely meridional observation, has conferred incalculable benefits on astronomy, and I wish to express my humble tribute of admiration of the wise self-denial which has characterised its administration, and which has been proof against the allurements of researches, the pursuit of which must have militated against the primary functions of the Institution, namely, that of laying, extending, and conserving the very foundations of the science.

Still the fact must be plainly recognised, that whilst one branch of astronomy is provided for in the most efficient and permanent manner, the other branch is left literally to chance. And the question I wish now to ask is, whether this is a satisfactory state of things; and if not satisfactory, whether the first astronomical body in the world cannot do something to amend it? I know that this question is very likely to raise a controversy as to the value of private exertions, and as to the risk of damping individual enterprise by the establishment of new and powerful official institutions. I therefore, at the outset, beg to express my great respect for the labours of those who have devoted their means and time, gratuitously, to the cultivation of science in whatever department. I should be very sorry to see the spirit of individual research, in which Englishmen exceed all other people, discouraged by the action of the community; but I have no fear of such a result. I believe that by a proper classification of objects, there will be left an ample field for private observers, even should official astronomy be very much extended.

It appears to me that this classification should be based on the principle that researches which involve long-continued, systematic labour, should be undertaken by permanent establishments. I would give, as a simple example of such researches, the study of the Sun—a study which, in all probability, will never come to an end. In such a study, well-considered measures, strictly and uninterruptedly persevered in for a long period of years, are absolutely indispensable. It is certain that we cannot count upon such extended continuity from private energy, which however supreme it may be, must die with its possessor.

On the other hand, the work which may be expected from volunteer astronomers consists chiefly in opening out new fields, and in devising new processes of inquiry. I may here again cite the same example of the Sun, the systematic study of which has

been rendered possible by the personal exertion and genius of men too well known to us all to need enumeration. But the work is now fast getting too heavy for private resources. The mapping of the Moon is another example of work which individuals have shown us how to do; but which individuals can never complete within any reasonable period.

My present object is to draw attention to the general question of the insufficiency of existing astronomical establishments, in order to elicit opinions on it. The details of any new scheme will be matters for future consideration. At present, I shall content myself with stating my opinion that permanent national provision for the cultivation of the Physics of Astronomy, is urgently needed. If the study of the Sun only were in question, that alone would, I think, justify such a measure; for there can hardly be a doubt that almost every natural phenomenon connected with climate can be distinctly traced to the Sun as the great dominating force, and the inference is unavoidable, that the changes, and what we now call the uncertainties of climate, are connected with the constant fluctuations which we know to be perpetually occurring in the Sun itself. The bearing of climatic changes on a vast array of problems connected with navigation, agriculture, and health, need but be mentioned to show the importance of seeking in the Sun, where they doubtless reside, for the causes that govern these changes. It is indeed my conviction, that of all the fields now open for scientific cultivation, there is not one which, quite apart from its transcendent philosophical interest, promises results of such high utilitarian value as the exhaustive, systematic study of the Sun.

The present seems to me an opportune time for agitating this question, for two reasons. In the first place, it has been recently announced that the famous series of Solar photographic researches conducted by Messrs. De La Rue, Balfour Stewart, and Loewy, have been brought by circumstances to a close. The changes daily in progress on the Sun's surface are now therefore no longer systematically depicted by photography anywhere in the British empire. The results obtained, as recorded by the three above-named astronomers in the *Transactions* of the Royal Society, are of such striking interest and importance, that any prolonged loss of continuity in the series will be a most deplorable circumstance. No time, therefore, should be lost in averting such an evil.

In the second place, it is, no doubt, generally known, that a Royal Commission, of which the Duke of Devonshire is Chairman, has been for about two years engaged in a twofold inquiry into "Scientific Instruction and the Advancement of Science." The first branch of their inquiry is completed, and they are about to enter on the second, namely, the "Advancement of Science." We may feel very sure that the present generation will not see so comprehensive an inquiry undertaken a second time, and that any conclusions this Commission may now come to will govern the

principles of State action with regard to science for many years to come. The opportunity, therefore, which is now open to the Astronomical Society to make any recommendations which in the present state of the science may seem to it advisable, is one which for a long period may not again be presented to it.

On the Law of Facility of Errors of Observations, and on the Method of Least Squares. By J. W. L. Glaisher, B.A., F.R.A.S., F.C.P.S., Fellow of Trinity College, Cambridge. (Abstract.)

The *American Journal of Science and Arts* for June 1871, contains a note by Professor Cleveland Abbe, the object of which is to point out that Professor Robert Adrain, of New Brunswick, published the method of Least Squares in 1808, having been independently led to its discovery. It is well known that all the proofs that have been given of the method of Least Squares, contain, to say the least, some points of difficulty, and on this account any new investigation of the result is necessarily a matter of much interest. Although some of the investigations of the law $e^{-h^2 x^2}$ are far from rigorous, still there is not one that is not of some importance, as throwing additional light on the properties of this law, so that a fresh investigation, and one moreover, by which the law, not previously known to the author, was discovered, might be suspected to be a real addition, or at all events, confirmation of the known processes. Dr. Adrain's investigation is therefore examined at some length; but such is not found to be the case; the reasoning being of so slight and inconclusive a nature as to lead one to believe that, like Legendre, Dr. Adrain first remarked the convenience of testing equations by the method of Least Squares, and subsequently endeavoured to justify this rule by the Theory of Probabilities. After noticing the way in which Legendre originally presented the method, the other investigations that have been given are examined in detail, being grouped as follows:—

1. Gauss's original investigation, including Encke's, De Morgan's, and Leslie Ellis's remarks on the principle of the Arithmetic mean.
2. Laplace's Method, including Poisson's and Ellis's Simplifications, and Ivory's Criticisms.
3. Gauss's Second Demonstration, and its connexion with Laplace's.
4. Sir John Herschel's proof, with Ellis's and Boole's Criticisms thereon.
5. Professor Tait's, and similar proofs (Quetelet's, Hagen's, &c.)
6. Donkin's Proof.

Besides the above, four investigations were given by Ivory of the method, of which only one is referred to in detail, the others having been discussed by Ellis. Memoirs by Bessel and Mr. Crofton are also referred to.

The principle of the arithmetic mean, regarded either as an axiom or a result of demonstration, is examined, and the author believes them to be untenable. Laplace's method, as extended by Ellis, is restated with some alterations, and Poisson's general theorem given in the *Connaisance des Tems* for 1827, is proved, starting at once from a multiple integral, which is evaluated by means of Lejeune-Dirichlet's principle. The awkward transition from finite to infinitesimal errors in the course of the analysis is thus avoided. The exact sense in which Laplace's system of factors gives the most probable values of the quantities to be determined, is examined in some detail, and attention is drawn to the case taken by Laplace, when the facilities $\phi_1(x)$, $\phi_2(x)$ are supposed known, and the most probable values of the unknowns are to be found; and the actual case when in effect $\phi_1(x)$, $\phi_2(x)$ as well as the most probable values, are to be found. The one case is, given the observations and weights, find the values; the other, given the observations, find the weights and values.

The method of Least Squares gives in fact only the first approximation to the true values, which, however, enables us to weigh the observations and so obtain a second approximation, and so on. A criticism like Professor Pierce's is false in principle: it may certainly be better to reject an observation entirely, than to give it a weight equal to that of the best; but a proper extension of the method of Least Squares would give an abnormal observation a very small weight. Reasons are given why Laplace's first view of the subject seems much preferable to his second (the disadvantages in which are considered), and therefore preferable to Gauss's second demonstration also. The two views of the matter taken by Laplace (in his first investigation), and by Gauss, lead to a theorem in multiple integrals, which is verified. The final result of the discussion of all the proofs of the method that have been given is, that the only correct and philosophical view of the subject is that which regards an error as formed by the aggregation of a great number of smaller errors, due to independent sources, and subject to arbitrary laws of facility, whence by Laplace's analysis (though it was not so applied by its author), we have generally the law $e^{-h^2x^2}$ for individual actual errors, and the rule of Least Squares follows at once from this law, so that if the number of observations be large, we have, as it were, a double reason for the method. The paper concludes with one or two curious conclusions that would follow if the law of facility were $e^{-m\sqrt{x^2}}$, a form which not only seems very natural, but which was actually assumed to be the true one by Laplace in one of his earlier memoirs.

The Solar Eclipse of December 12th, 1871.
By Mr. Tebbutt.

The delicate state of my health prevented me from joining the expedition for observing the total eclipse at Cape Sidmouth; but I availed myself of the instrumental appliances at Windsor, for the observation of the partial phase. It unfortunately happened that the evening of the 11th was cloudy, so that no observations could then be taken for the determination of the mean time at the Observatory. The following morning, however, broke clear and fine, and I succeeded some hours after sunrise in observing the transits of two stars of the first magnitude. These observations in connexion with others taken in the evenings of the same and the following day, served for the determination of the chronometer errors and rate. A strong N.W. wind prevailed throughout the day of the eclipse, and broken clouds passed rapidly across the sky during the forenoon. With the exception of two or three small fleecy clouds, which passed almost instantaneously over the disc, and that without appreciably obscuring it, the Sun was bright and unclouded throughout the phenomenon. There were several spots on the Sun, but they were not of a very remarkable character. A group of small spots was to be seen on the eastern limb, involved in an immense tract of faculæ. The first contact appeared to take place at $2^h\ 18^m\ 39^s{\cdot}1$, owing, however, to a momentary removal of the eye from the telescope, this phase of the eclipse must have been observed somewhat late, probably to the extent of two or three seconds. The Sun's limb was remarkably steady and sharply defined. A very small sun-spot was observed to disappear at the Moon's edge, at $2^h\ 40^m\ 44^s{\cdot}5$, and at $2^h\ 57^m$, the same spot was again seen immediately after it had emerged from the Moon's limb. The most conspicuous spot on the disc was bisected by the Moon's limb at $3^h\ 28^m\ 41^s{\cdot}9$. Up to $3^h\ 30^m$, the images were very steady, and the cusps sharply defined; there being no rounding-off of the latter at the extreme points. At this time, however, the Sun's limb began to boil, which disturbance increased rapidly at $3^h\ 36^m$. The large conspicuous spot was again bisected at $3^h\ 37^m\ 24^s{\cdot}4$, while emerging from the Moon's limb. At $3^h\ 43^m$, the limb approached very near to the large tract of faculæ, but did not pass over it. During the early progress of the eclipse, the mountain-peaks and ranges along the edge of the Moon were beautifully distinct against the solar background. I believe I never before saw them to such advantage. Towards the end of the eclipse the images became more steady. The last contact took place at $4^h\ 4^m\ 38^s{\cdot}3$. The most interesting circumstance perhaps in connexion with the eclipse was the effect which the phenomenon had on the vacuum solar thermometer, suspended freely in the Sun's rays. From the beginning of the eclipse the thermometer gradually sank till about the time of greatest obscuration, when it began to rise, and continued to do so till the end of the phenomenon. The

instrument then, owing to the decreasing altitude of the Sun, began to fall again.

As the readings of the thermometer may prove interesting, I subjoin them:—

Readings of the Solar Thermometer during the Eclipse, December 12th, 1871.

Mean Time. h m s	Therm. deg.	Mean Time. h m s	Therm. deg.	Mean Time. h m s	Therm. deg.	Mean Time. h m s	Therm. deg.
2 22 25	114·8	3 5 25	107·4	3 46 40	109·5	4 18 10	112·1
2 24 55	114·3	3 8 55	107·0	3 49 55	110·0	4 19 55	111·8
2 32 25	113·6	3 12 25	106·3	3 52 55	110·5	4 22 25	111·1
2 36 25	112·6	3 15 55	106·1	3 55 40	111·0	4 24 55	111·0
2 41 55	111·5	3 19 55	106·0	3 59 55	111·8	4 27 25	110·4
2 44 55	111·1	3 22 55	106·1	4 2 25	112·8	4 29 55	109·7
2 48 55	110·5	3 25 55	106·3	4 5 55	113·5	4 32 25	109·2
2 52 55	109·6	3 29 55	106·6	4 9 25	113·3	4 34 55	109·0
2 55 25	109·0	3 33 55	107·3	4 11 55	113·0	4 50 55	107·0
3 0 25	107·7	3 39 55	108·7	4 13 55	112·9	5 27 25	103·8
3 3 25	107·2	3 43 40	109·0	4 15 55	112·5	6 16 25	93·0

On the Aurora of February 4. By H. P. Finlayson.

As it is probable that some members of the Royal Astronomical Society saw the Aurora Borealis of the evening of Sunday last, Feb. 4th, I am anxious to record its appearance as seen here; for, should the display have been similar to an observer at some considerable distance from Dover, it may be possible thereby to obtain some faint idea at what height such an electric storm takes place. The fore part of the evening was cloudy, and no stars visible; the clouds close to the Earth seemed vapoury, or large foggy clouds, and were reflecting a blood-red colour, as from some great conflagration. One could not tell how this red light was obtained, for above them there appeared to be one large cloud that covered the whole of the sky, thus preventing the stars being seen between the openings of the lower clouds. This led me to think that the cause of this red light must be very close to the Earth, on a level with or just above the lower clouds. This, however, cannot be correct, since the phenomenon was seen in Paris and Constantinople.

Between 10 and 11 the sky was quite clear of clouds, and then the Aurora Borealis was seen in perfection,—rays of light in every direction, and all radiating from one point in the heavens. At one time there were so many streamers, and the radiation so complete, that the point of departure was pretty accurately

given; it was in the constellation *Gemini*, and between *Jupiter* and the two principal stars.

On intently watching these rays of *white* light, I found they had a slow motion in the same direction as the Earth on its axis: thus showing that the whole must have been revolving very slowly round the radiating point, taking, I should think, five or six hours to complete a revolution. Many of the rays were 60° to 70° long, and running direct from the centre. The light of the stars was very little interfered with when a streamer passed over them: I should say not at all.

Now, should you have any other description of this Aurora Borealis and the radiant point altogether different, it would give us some clue whether all this emanates from the atmosphere or above it.

At the beginning of last month I noticed a very large halo round the Moon, quite 40° to 60° in diameter, a complete circle; and yet the Moon was very little more than a thin crescent. This I thought strange and curious, and, thinking it may prove interesting, have mentioned it here.

Dover, Feb. 8, 1872.

Occultation of ζ Tauri, December 8th, 1870. By J. Maguire, Esq.

In the last Supplementary Number of the *Monthly Notices* there is a communication from the Rev. J. Spear, from Churkrata, in India, in which the following statement occurs:—

"I am sorry I have not been able to record any observations of importance, except perhaps the error in the calculation of the occultation of ζ *Tauri*."

"December 8. Watched for the occultation of ζ *Tauri*. Greenwich mean time of apparent conjunction in right ascension of Moon and star $5^h\ 0^m\ 3^s$. Limiting parallels N 90–N 19. No occurrence. The Moon passed at least 10′ north of the star to the best of my judgment."

The geographical position given is—

Latitude ..	..	..	30° 42′ 4″
Longitude..	..	..	$5^h\ 11^m\ 42^s$

I may add that the right ascension at conjunction is $5^h\ 0^m\ 39^s$ in the *Nautical Almanac*, and the latitude, although not marked N., I have assumed to be so.

If, then, the error above mentioned refers to what took place on the 8th December, 1870, it appears that Mr. Spear, finding

himself well within the limiting parallels, watched with confidence for the occultation of ζ *Tauri*. But his surprise may be imagined when he found that the Moon's lower limb at its nearest approach was in his estimation at least 10′ north of the star. He records the words "no occurrence," and leaves us to infer that he has discovered an important error in the *Nautical Almanac*.

If there be many observers who take it for granted that an occultation must occur at all latitudes within the limiting parallels, then I fear the *Nautical Almanac* will get a bad name.*

I have calculated the limiting parallels, which I find the same as in the *Nautical Almanac*. And a calculation for the position given above shows that the nearest approach of the Moon's centre to the star was 19′·3. The moon's lower limb was, therefore, within 4′·5 of the star. This differs materially from Mr. Spear's 10′. The question, however, is not one of proximity, but whether a limiting parallel in such a case as this is a delusion and a snare.

Norwich, Nov. 13, 1871.

Discovery of the Minor Planet (118) *Peitho.*

This planet was discovered by Dr. R. Luther, at Bilk, near Dusseldorf, on March 15, 1872. It was of the eleventh magnitude. The following elements have been calculated by Professor Theodore von Oppolzer, from observations made on March 15, 26, and April 4:—

Epoch, 1872, March 31·0, Berlin Mean Time.

$$\begin{array}{lcl} L & = & 160^\circ\ 53'\ 44''\!\cdot\!7 \\ M & = & 84\ 25\ 12\cdot3 \\ \varpi & = & 76\ 28\ 32\cdot4 \\ \text{Node} & = & 47\ 14\ 25\cdot5 \\ \iota & = & 7\ 50\ 11\cdot3 \\ \phi & = & 9\ 51\ 25\cdot0 \\ \mu & = & 928''\!\cdot\!402 \\ \log a & = & 0\cdot388181 \end{array} \quad \left.\begin{array}{l} \\ \\ \\ \\ \\ \\ \\ \\ \end{array}\right. $$

(ϖ, Node, ι: Mean Equinox 1872·0)

Sidereal revolution = 1396 days.

[* In the Explanation of the Elements of Occultations, at the end of the *Nautical Almanac*, it is stated that "by Limiting Parallels are to be understood those parallels of latitude beyond which an occultation cannot *possibly* occur. . . . Limiting parallels are useful to indicate whether, at a given conjunction of a star with the Moon, the positions are likely to produce an occultation in a given latitude, and thus to save considerable labour to the computer." Ed.]

The following ephemeris may serve for comparison with the observations:—

12h Berlin M.T.	R.A. h m s	N.P.D. ° ′	Log Δ.
April 4	11 48 22	78 50·7	
8	11 45 11	78 48·8	0·194
12	11 42 22	78 50·8	
16	11 39 55	78 54·5	0·212
20	11 37 54	79 1·9	
24	11 36 18	79 12·0	0·232

Peitho is the nineteenth minor planet discovered by Dr. R. Luther.

April 10, 1872.

Observations of the small Planets Peitho (118) *and Egina* (91), *of six new Nebulæ, and a Variable Star.* By M. Alph. Borrelly, Assistant Astronomer at the Marseilles Observatory.

(*A Letter to the President.*)

I have the honour of forwarding to you two observations of the small planets *Peitho* (118) and *Egina* (91). To these I add, in like manner, the positions of six new nebulæ, and of a variable star.

Observation of the Planet (118) *Peitho.*

	Marseilles M.T.	App. R.A.	Log. f. par.	App. Decl.	Log. f. par.
1872, Mar. 27	$8^h44^m35^s$	$11^h55^m41^s{\cdot}75$	1·4501	$+10°56'19''{\cdot}7$	0·7012

Comparison Star.

	Magnitude.	Mean R.A. for 1872·0.	Correction for Date.	Mean Decl. for 1872·0.	Correction for Date.
932 Weisse, H. 11	9	$11^h54^m58^s{\cdot}51$	$+0^s{\cdot}98$	$+11°3'56''{\cdot}85$	$-5''{\cdot}48$

The planet is of 11–12 magnitude.

Observations of the Planet (91) *Egina.*

	Marseilles M.T.	App. R.A.	App. Decl.
1872, March 13	$10^h30^m57^s$	10^h 02	$+14°7'35''{\cdot}9$

Comparison Star.

	Magnitude.	Mean R.A. for 1872·0.	Correction for Date.	Mean Decl. for 1872·0.	Correction for Date.
3475 B.A.C.	6	$10^h4^m45^s{\cdot}16$	$+0^s{\cdot}82$	$+13°59'9''{\cdot}13$	$-2''{\cdot}6$

The planet is of the 11th magnitude.

New Nebulæ discovered and observed by Alph. Borrelly at the Marseilles Observatory, with the Eichens Searcher.

Mean Positions for 1872·0.

Name of Comp. Star.	R.A. h m s	N.P.D. ° ′ ″	Remarks.
a	6 39 21·87	5 26 3·7	Nebula pretty faint, extended, elliptic, no bright point.
b	6 48 44·78	4 4 20·2	Nebula pretty bright, moderately extended, round; nucleus of 12–13 mag.
c	8 51 44·10	11 25 18·2	Nebula pretty bright, 3′ in extent, elliptic; no nucleus.
d	11 37 50·45	72 43 31·6	Nebula round, little extended; small nucleus at centre.
e	11 45 26·37	72 28 20·3	Nebula pretty faint, extended, elliptic; no bright point.
e	11 45 35·87	72 25 46·1	Nebula exceedingly faint, nearly round, almost undiscernible.

Mean Positions of the Comparison Stars for 1872·0.

	Name of Stag.	Mag.	R.A. h m s	N.P.D. ° ′ ″
a	374 Œltzen	7·5	6 25 56·27	5 11 46·4
b	381 Œltzen	7·8	6 41 8·55	4 3 39·8
c	1373 Fedorenko	8·0	8 41 35·40	11 22 51·2
d	666 Weisse, hora 11	9·0	11 35 5·45	72 37 6·2
e	889 Weisse, hora 11	8·9	11 46 36·24	72 26 11·8

Variable Star.

On November 3, 1871, a star in R.A. $0^h 17^m 17^s{\cdot}68$ and N.P.D. $100° 10' 10''{\cdot}1$ (mean position for 1872·0) appeared to be of magnitude 6–7. On the 8th it was of the 8th magnitude. On the 24th I noted it as of the 10th magnitude. From November 30 until January 1872 I noticed no change. Since then I have been unable to observe it.

Discovery of Minor Planets (119) *and* (120).

A communication has been received from M. Delaunay, Director of the Observatory of Paris, announcing the discovery, by M. Paul Henry, of Paris, of Minor Planet (119). It was first observed on the 9th of April, at 11 P.M.; but the observation then obtained was only approximate. A second observation was made on the next night by M. Prosper Henry, with the equatoreal, in the garden of the Observatory of Paris.

	Paris M.T. h m s	R.A. h m s	N.P.D. ° ′ ″
1872, April 9	11 0 0	13 18 59	98 40 23
10	12 41 49	13 18 4·61	..
10	12 56 0	..	98 37 34·0

The hourly motion in R.A. is $-1^s{\cdot}75$, and in N.P.D. —25″ The planet is of the 11th magnitude.

The following telegram has been received by M. Delaunay from M. Borrelly, of Marseilles :—

"I have discovered this evening at 11^h, a new Minor Planet (120), the observed positions of which are as follows :—

	Marseilles M.T.	App. R.A.	App. N.P.D.
	h m s	h m s	° ′ ″
1872, April 10	12 16 32·0	12 0 55·38	95 2 44·9
10	13 14 36·5	12 0 53·63	95 2 41·4

Comparison Star.

In a letter addressed to the President, M. Borrelly gives the following additional particulars :—

	Magnitude.	Mean R.A. for 1872·0.	Correction for Date.	Mean N.P.D. for 1872·0.	Correction for Date.
994 Weisse, hora 11	7–8	$11^h 59^m 1^s·96$	$+0^s·93$	95° 7′ 58″·64	+6″·73

The planet is equal in brightness to a star of 11-12 magnitude.

April 12, 1872.

Observations of Encke's Comet, at Mr. Bishop's Observatory, Twickenham. By Mr. Hind.

The observations marked H are by Mr. Hind; those marked P by Mr. William E. Plummer. The place of October 8 is uncertain from the faintness of the comet.

1871.	Twickenham M.T.	R.A.	Log. Fact. Par.	Decl.	Log. Fact. Par.	
	h m s	h m s		° ′ ″		
Oct. 8	7 41 8	1 23 2·4:	−8·6789	+35 39 9:	9·6990	H
10	10 30 42	1 15 29·1	−8·2895	36 15 36	9·4565	H
12	8 2 6	1 7 49·12	−8·6313	36 47 12·6	9·6139	P
	9 16 18	1 7 37·76	−8·4945	36 47 38·4	9·5115	H
14	8 28 24	0 58 49·29	−8·5774	37 18 32·6	9·5499	P
18	9 1 22	0 37 33·.8	−8·3868	38 13 52·0	9·4005	—
Nov. 5	10 3 38	22 7 19·62	+8·5488	35 22 38·0	9·5587	H
15	6 53 21	20 32 6·89	+8·3630	25 27 51·1	9·6701	P
16	7 30 41	20 23 4·37	+8·4883	24 10 32·7	9·7177	H
17	7 6 13	20 14 36·51	+8·4504	22 54 5·3	9·7228	P
18	6 59 1	20 6 13·67	+8·4864	21 35 24·7	9·7410	—
20	7 48 52	19 49 42·28	+8·5807	18 51 54·0	9·5047	—
Dec. 2	5 4 18	18 27 19·41	+8·5067	3 3 34·8	9·8794	H
3	5 15 28	18 21 15·82	+8·5371	1 47 46·1	9·8856	—
4	5 12 5	15 23·01	+8·5463	0 33 52·6	9·6110	P
	5 27 57	18 15 20·41	+8·5630	+0 33 10·7	9·8905	H
5	5 16 4	18 9 36·76	+8·5618	−0 38 50·3	9·6336	P
6	5 17 22	18 3 57·71	+8·5728	−1 50 48·9	9·6496	—

B

Orbit of the Binary Star Σ 1938 *near* μ^2 *Boötis.* By Mr. Hind.

The following elements fairly represent the measures of this star up to the date of nearest approach of the components:—

T	....	1860·88
Node	....	163°11′
λ	....	54 27
γ	....	41 52
$\phi = \sin^{-1} e$		34 20·4
a	....	1″·761

Period of Revolution, $314^{yrs.}$·34

For 1865·46	Dawes	$\theta\,(c - o)$	$= -1°·86$
1867·56	Engelmann		$= +1\ ·02$

In this orbit the angle and distance are thus given:—

	°	″
1872·0	166·28	0·880
73·0	163·52	0·902
74·0	160·90	0·920
75·0	158·38	0·931
76·0	155·97	0·935

Orbit of the Binary Star ξ *Boötis.* By Mr. Hind.

Elements of this star were first given by Sir John Herschel in the *Memoirs* of this Society, and an orbit has also been computed by Mädler. The following elements appear to represent the whole course of measures better than any previously published:—

T	....	1779·75
Node	....	11°23′
λ	....	96 25
γ	....	71 36
ϕ	....	51 28
a	....	9″·95

Period, $168^{yrs.}$·91 $\log \mu_0 = -0·32864$

Hence the angles of positions and distances are—

	°	″
1872·0	291·25	5·638
73·0	289·95	5·613
74·0	288·63	5·587
75·0	287·29	5·561
76·0	285·94	5·535

The measures of ξ *Ursæ Majoris*, published in the *Monthly Notice* for March by Mr. Knott, indicate that the peri-astron passage will fall earlier than assigned in the elaborately worked orbit of M. Yvon Villarceau (*Conn. des Temps*, 1852). The mean of Mr. Knott's measures give an angle of position 33° in advance of that computed. The angle in Capt. Jacob's orbit (*Mem. R. Astron. Soc.*, vol. xvi.) is 26° behind the observed one. Observations during the next few years will have great value for the better determination of the orbit of this star, which, as being the first submitted to calculation by M. Savary, possesses an especial interest.

On the large number of Stars visible to the Naked Eye in the Southern Heavens. By Richard A. Proctor, B.A. (Cambridge.)

Dr. Gould makes the following remarks in his address at the inauguration of the Argentine Observatory at Cordoba: "The transparency of the sky of Cordoba upon favourable nights may be judged of by a single additional fact. You will find in the treatises on astronomy the total number of stars in the entire heavens visible to the naked eye estimated at from 5500 to 6000. Now, we have already recorded in the *Uranometria Argentina* the places of not less than 6400 stars, visible to the unaided sight of every one of our observers, in the Southern Hemisphere alone; while in the first 10° of the Northern Hemisphere we have 800 more, making in all at least 7200 stars; so that we are justified in the belief that were the sky equally transparent for astronomers in the Northern Hemisphere, the total number of stars visible to the ordinary eye would be estimated at certainly not less than 11,000, instead of half that number."

It seems to me, that until the contrary has been demonstrated by some independent evidence, the circumstances recorded by Dr. Gould should be held to show that the southern skies are really richer in stars than the northern. It may be remembered that in the preface to my large Star Atlas I had already indicated my belief that there is an aggregation of lucid stars in the southern heavens,—this aggregation being however local, insomuch that certain parts of the southern heavens are exceedingly poor in stars, while others are as remarkably rich. Now, if Dr. Gould finds, on examining the arrangement of the 6400 stars observed at Cordoba, that the local aggregation I have spoken of exists,—a result of which I do not entertain the slightest doubt,—it will be evident that what is in question is a true law of stellar distribution, and not the greater transparency of southern skies.

I must confess I know of no evidence whatever tending to show that, *cæteris paribus*, the skies are more transparent in the

southern than in the northern hemisphere. Sir J. Herschel, by proposing to continue his photometric experiments on the stars during the whole continuance of his voyage home from the Cape, implied that he at least recognised no such distinction. Why, indeed, should the southern skies be more transparent than the northern? It is true that in passing southwards from our northern latitudes we gain regions where the skies are purer; but would not the inhabitant of corresponding southern latitudes find a corresponding change as he travelled northwards? The low barometer of high antarctic latitudes will not account for the supposed difference, and the greater extent of ocean surface in the southern hemisphere must far more importantly tend to render the southern skies less pure.

MONTHLY NOTICES

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PROFESSOR CAYLEY, President, in the Chair.

Report on Observations made by the Government of India on the Total Eclipse of the Sun on Dec. 11–12, 1871. By Lieut.-Col. Tennant, R.E., F.R.S. (Abstract.)

This paper is an official report on the observations of the Total Eclipse of December last, which was observed by Lieut.-Col. Tennant and his coadjutors by the orders of the Indian Government.

A letter to Dr. Huggins, published at page 70 of the present volume of the *Monthly Notices*, will have given the Fellows of the Society some knowledge of what was observed, and as Col. Tennant's paper will appear at greater length it will not be necessary to make this notice very long.

Dodabetta, the station of observation, is the principal peak of the Nilgherry Hills, situated in N. Lat. 11° 24′, E. Long. 76° 43′. Its height above the sea is 8650 feet nearly, and the observations made here, and especially the photographs (of which glass transparencies were exhibited at the meeting), are valuable for comparison with those of the observers of the British expedition whose stations were at a far lower level.

Col. Tennant expresses himself very positively as to the absence of any well-defined and nearly uniformly bright portion of the corona whose extent is limited to some four or five minutes from the Sun; up to the limit to which it was examined the light

faded away. He also is confident that those portions of the corona to which his attention was directed were constant, and this is evidenced not only by his own photographs but by comparison with the pictures taken by Lord Lindsay's assistant, Mr. Davis, at Bekul, as there exists the most marked resemblance.

The spectroscopic results are described as in the paper before mentioned. Identifications of the lines are given, but of course these are liable to some considerable uncertainty. The prominent point is that the green line considered to be identical with K 1474 was apparently continuous across the rifts. Of course there was no possibility of recording the numerous lines which Capt. Herschel saw as the solar limb appeared. It is the fact that they were so numerous which is of importance.

As regards the photography the report gives some details as to the procedure, and then proceeds to describe the photographs. These descriptions would hardly be intelligible without the pictures themselves, copies from which were examined with much interest by many of the Fellows. The corona is extremely beautifully depicted in the four early photographs, while the fifth, though less sharply defined, is still exceedingly good. The sixth is defective, evidently from the clock having ceased to act; and as it was stated that it was going after the set was completed, it seems not improbable that one of the clamps which had slipped once before had again temporarily yielded. The encroachment of the corona and prominences on the lunar limb is very marked, and it produces the *elongated appearance* of the Moon which Colonel Tennant, in his letter to Dr. Huggins, mentions having noticed in the hurried examination he was able to give before it left. The transparencies on opal glass were much admired, and do great credit to Captain Waterhouse, who made them from the negatives, which are themselves the result of his manipulation.

As regards Polariscope observations, Col. Tennant gives a result by Mr. Broughton, B. Sc. London, who found no polarization before total phase, but that Savart's Polariscope showed the characteristic bands brightly at the instant when the coronal light appeared.

Colonel Tennant concludes by giving the results at which he has arrived from an examination of the results reported in this and his preceding paper, and a comparison with those known to him as having been attained by others. He says:—

"The following, then, seems to be the constitution of our Sun. There is a nucleus which gives out continuous white light like solid or liquid bodies, and even dense gases. Surrounding this is a layer of heavy vapours intensely heated, but far less so than the nucleus, and in which, if a state of equilibrium could exist, the heaviest vapours would be lowest. Above this is a layer of glowing hydrogen of very slight density accompanied by that gas which gives out the line D_3. Still further up these gases in a cooler state become mixed with what gives out the green line K 1474; and, lastly, that alone seems to remain."

Colonel Tennant has deposited the original negatives of the photographs taken on this occasion with the Astronomer Royal.

On Errors in Vlacq's (often called Briggs' or Neper's) Table of Ten-figure Logarithms of Numbers. By J. W. L. Glaisher, B.A., F.R.A.S., Fellow of Trinity College, Cambridge.

As, with the exception of Vega's *Thesaurus &c.*, the only complete table of ten-figure logarithms that has been published is the original one, partially calculated by Briggs and completed by Vlacq, the publication of errors detected in this work is of very considerable importance. Lists of errata have been given by Vlacq himself, by Vega, Sherwin, Lefort, and others, but it is not possible to find without comparison to what extent these lists are concurrent or supplementary; the main object of this note is, therefore, to give the results of an examination of these tables of errata, and to supplement them with some not previously published. In order to render clear what follows, it is necessary to premise some facts with regard to the original calculation of the logarithms of numbers.

The first table of logarithms to the base 10 was calculated by Henry Briggs, Savilian Professor of Geometry at Oxford, and published by him under the title, *Arithmetica logarithmica, sive logarithmorum chiliades triginta, pro numeris naturali serie crescentibus ab unitate ad* 20,000*: et a* 90,000 *ad* 100,000. . . . *Londini, excudebat Gulielmus Jones*, 1624.* This table contains the logarithms of the natural numbers from unity to 20,000, and from 90,000 to 100,000 to 14 places of decimals. There is thus left a gap from 20,000 to 90,000, which was filled up by Adrian Vlacq, who published at Gouda, in 1628, a table containing the logarithms of the numbers from unity to 100,000 to 10 places of decimals. Having calculated 70,000 logarithms and copied only 30,000, Vlacq would have been quite entitled to have called his a new work. He designates it, however, only a second edition of Briggs, the title running, *Arithmetica logarithmica, sive logarithmorum chiliades centum, pro numeris naturali serie crescentibus ab Unitate ad* 100,000. . . . *Editio secunda aucta per Adrianum Vlacq, Goudanum.* . . . *Goudæ excudebat Petrus Rammasenius*, 1628. This table of Vlacq's was published, with an English explanation prefixed, in London in 1631. The title of the English work is, *Logarithmicall Arithmetike;*

* Some copies, with the same title-page, contain an additional chiliad; viz., the logarithm of numbers from 100,000 to 101,000. The library of Trinity College, Cambridge, contains one of these copies, which belonged to Dr. Brinkley. In 1617 Briggs published his *Logarithmorum chilias prima.*

or, Tables of Logarithmes for absolute numbers, from an unite to 100,000. . . . . *London, printed for George Miller*, 1631.

Speaking of Briggs' *Arithmetica Logarithmica* of 1624, De Morgan, in his article on Tables in the English Cyclopædia, says, "After his [Briggs'] death, in 1631, a reprint was, it is said, made by one George Miller; the Latin title and explanatory parts were replaced by English ones—'Logarithmicall Arithmetike,' &c. We much doubt the reprint of the tables, and think that they were Briggs' own tables, with an English explanation prefixed in place of the Latin one. Wilson (in his *History of Navigation*, prefixed to the third edition of Robertson) says, that some copies of Vlacq of 1628 were purchased by our booksellers, and published at London with an English explanation premised, dated 1631. Mr. Babbage (to whose large and rare collection of tables we were much indebted in the original article) has one of these copies; and the English explanation and title is the same as that which was in the same year attached to the asserted reprint of Briggs. We have no doubt that Briggs and Vlacq were served exactly in the same manner." On referring to Robertson (Fourth Edition, p. xvi.), I find no further information than that contained in the above extract. That the above suggestion of De Morgan's is correct, and that Miller's and Vlacq's tables are both printed from the same types, is evident on examination. Miller's copies are not quite so rare as they might be inferred to be from the above remarks; there is one in the University Library, Cambridge, and one in the library of Trinity College, Cambridge; while the library of the Royal Observatory at Greenwich contains two; there is, however, no copy in the libraries of the Royal Society or the Royal Astronomical Society. On the fly-leaf of the first of these copies (viz. that in the University Library) there is written in pencil, "N.B. These tables were edited by Vlacq and printed at Gouda." Three out of the four copies mentioned (the Trinity one is the exception) contain, after the English introduction and before the tables, a title-page, on which is printed, *Tafel der Logarithmi voor de Ghetallen van* 1 *af tot* 100,000, while the corresponding page in Vlacq's table is inscribed, *Chiliades centum Logarithmorum pro numerus* (sic) *ab unitate ad* 100,000. This shows the Dutch origin of Miller's table; and it is besides curious, as it seems to imply that Vlacq meditated a Dutch translation of his work, but that the tables intended for the purpose were bought and published by George Miller. A careful examination of any one page of Vlacq's or Miller's copies is sufficient to show that they were printed from the same types; a good confirmation is afforded by the following error: in the numbers 3376, 3377 . . . . 3380 were printed instead of 4776, 4777 . . . . 4780; to remedy this a small piece of paper, on which the latter numbers were printed, was stuck over the former, and this occurs in both Vlacq's and Miller's copies. This and similar facts place beyond all doubt the identity of the tables, whether published in Gouda or London, and to

establish this fact is important, as we are thus assured that the same errata-list suffices for both. It is this table (which in Miller's copies, from the occurrence of both names on the title-page, is generally called after Briggs or Neper, and in the original copies after Vlacq) which was referred to at the beginning of this note.

In 1631 Vlacq published his *Trigonometria Artificialis*. This work contains, among other tables, the logarithms of the numbers from unity to 20,000, printed also (with the exception of the last sheet referred to further on) from the same types.

No further calculation of logarithms of numbers took place till the end of the last century, when the great French manuscript tables (the *Tables du Cadastre*) were computed under the direction of Prony. These, as is well known, have never been published.

The lists of errata which will be particularly referred to are,—that prefixed to Vlacq's own table in the *Arithmetica Logarithmica*,* that given by Vlacq on the last page of the table of the logarithms of numbers in the *Trigonometria Artificialis*, that given by Sherwin, and the elaborate table of M. Lefort, in t. IV. of the *Annales de l'Observatoire de Paris*, pp. [148]-[150], which was obtained by comparison with the *Tables du Cadastre*. This list also contains the errors given by Vega in the *Thesaurus logarithmorum completus*. It is intended to be supplementary to Vlacq's list in the *Arithmetica*, so that the two taken conjointly should give all the errors. One of the copies in the Greenwich Observatory library belonged to Maskelyne, and the following note is written on the cover: "All the errata have been corrected that had been discovered by, and are contained in William Gardiner's book (the author of the logarithm sines, &c., to every 10 seconds). Others, 22 in number, marked T, have been corrected, which were discovered by Michael Taylor, author of the logarithm sines and tangents to every second of the quadrant. Nevil Maskelyne, Astron. Regius, March 23, 1790."

I do not know where the errata found by Gardiner and Taylor were published, if at all: Gardiner's are not given in his tables. It is very probable that Taylor's were communicated personally to Maskelyne; it is worth notice that though the above note is dated 1790, yet Taylor's logarithm tables were not published till 1792. I have made no special search for either of the lists referred to, as the correction of the errors in the volume was sufficient for the purpose I had in view; viz., the discovery of errors not included in the lists of Vlacq and Lefort. The result of a careful comparison between Maskelyne's copy and Vlacq's and Lefort's list of errata was the detection of the following seventeen errors, not given by Vlacq in the *Arithmetica*, nor by Lefort.

* This list does not occur in Miller's copies.

Number.	Error.	Correction.
1360	89083	89084
1622 Diff.	26,76609	26,76699
2154	56970	56990
3192	13,62358	13,60358
7117 Diff.	6,10169	6,10179
9329 Diff.	4,75506	4,65506
9610 Diff.	4,51869	4,51896
*11275	65506	65505
11293	93280	93281
11699	87409	87410
18723	54375	54373
18724	86323	86325
23999	4,34019	4,38019
24580	18785	18786
53250 Diff.	81555	81557
*54040	53404	53403
64818	56295	56265

In the case of the two numbers marked with an asterisk the error is of no importance whatever, and not worth correcting; thus, the logarithm of 11275 is 4·05211, 65505, 49998, 14 . . ., and it is a matter of indifference whether the tenth figure of the mantissa be increased or not; log. 54040 = 4·73271, 53403, 49992, 98 . . ., and the same remark applies. These two logarithms were easily calculated by means of the logarithms up to 1200 to 20 places given by Callet, since 11275=25×451 and 54040=280×193.

On subtracting the logarithm of 14485 from that of 14486, the difference is 299814 instead of 299813, and Maskelyne's copy is accordingly so corrected; the book is, however, correct, as on referring to Briggs' table of 1624 the true difference is 2998132 . . ., the unit difference being due to the correction of the last figures of the logarithms in Vlacq. All the errata in the above list I have examined carefully, and satisfied myself that they are accurate. The errors corresponding to the numbers 1622 and 7117 also occur in Briggs' table of 1624.

The following are errata in Lefort's errata-table. Corresponding to the number 63747 the number in the error column should be 974 instead of 947. For the number 53053, the correction there given is 3 instead of 2 in the last figure, but the logarithm ends with a 1 and not a 3, and on calculating it from Callet (53053=77×689) I find that it is correct in the book. The correction probably has reference to some other number. For the number 50996 the correction 4 is printed so faintly as to be almost illegible, and for 16399 and 16699 the letter N is omitted.

Vlacq's errata-list in the *Arithmetica* contains no error; the

correction belonging to the number 14694 is superfluous in all the copies I have seen, the difference being printed correctly.

The following facts may be of interest: Vlacq's list in the *Arithmetica* contains 120 errata (106 of which correspond to numbers below 20,000). These are all corrected in Maskelyne's copy.* Lefort's list contains 452 errata, 301 of which are marked with an asterisk, to imply that they were given by Vega. Out of the 452, however, 287 may be called unimportant, denoting by this word errors which only alter by unity the last figure of the logarithm. There are 265 errors given by Lefort, not corrected in Maskelyne's copy, of these 222 were unimportant, and only 43 serious. There were, however, three corrections in Maskelyne's copy that were inaccurate, the original number being correct. The list in the *Trigonometria* (which only gives the logarithms of numbers from unity to 20,000) contains 79 errata in the logarithms and 40 in the differences; of the former three are not given in the *Arithmetica* (viz., those for the numbers 6197, 9182, 9429); they are to be found in Lefort. Of the errors in the differences 15 are new, while 5 (viz., those for the numbers 11400, 11499, 12582, 13639, 19081) which are given in the *Arithmetica* are omitted. Why these were not included I cannot guess; they are all important and uncorrected in the *Trigonometria*. The sheet (signature M) giving the numbers beyond 19500 is reprinted for the *Trigonometria*, and the error in 19960 (number) is corrected.

At the end of the preface to Sherwin's Tables† is a list of errata, those not given by Vlacq being marked with an "a." The error for the number 2167 should not be so marked, as it is to be found in both the *Arithmetica* and *Trigonometria;* three (viz., those for the numbers 6197, 9182, 9429) are in the *Trigonometria* and not in the *Arithmetica;* they therefore appear also in Lefort. The corrections belonging to the numbers 56359 and 57756, though marked with an "a," are not to be found in Lefort; they are, however, superfluous, as the logarithms are printed correctly in the copies I have seen: it is probable that they referred to indistinct figures in the copy Sherwin made use of. On the whole, Sherwin added twenty-one errata, of which one was not new and two were superfluous; the remaining eighteen are to be found in Lefort. Sherwin's list only gives errata in logarithms, not in differences. I have not made an examination of Vega's errata-list, feeling no doubt that this was completely done by Lefort.

Having found an error in one of the numbers, which was not

* In one instance the correction was wrongly made.

† Of Sherwin's Tables, De Morgan remarks in the article previously cited. "Second edition, 1717; third revised by Gardiner, and the best, 1742; fifth and last, 1771, very erroneous—the most inaccurate table Hatton ever met with." My father's copy, the errata-list in which I used in the above comparison, is dated 1726, and contains a list of "errata for the second edition of Sherwin's Mathematical Tables," by Gardiner.

given in any of the above errata-lists, it occurred to me that errors in the numbers would have stood a far better chance of escaping detection than errors in the logarithms, as the latter would be examined more or less by all succeeding editors of logarithmic tables; I have therefore made an examination of the numbers from 1 to 100,000 to discover misprints. The numbers from unity to 21,500 I examined myself, the rest were done for me by an assistant. The result is the discovery of errors in the following numbers:—

969	*16699
2081	19960
2082	34163
2260	49831
2278	*66150
3500	*66789
7368	*66790
12301	*69489
12345	*69490
16327	77052
*16399	

It is unnecessary to give the errors in the above numbers; the correction being, of course, evident at sight. The corrections for the seven numbers to which the asterisk is prefixed are contained in Lefort. Of the above twenty-one errors, twelve are corrected in Maskelyne's copy. It is probable that Vlacq's list in the *Arithmetica*, Lefort's, and the two lists in this paper, taken conjointly, contain nearly, if not quite, all the errors in Vlacq's tables. The total number is thus 603, of which about one-half are unimportant; the tables contain over 2,100,000 figures liable to error, so that there is one error, on the average, to every 3500 figures, or an important error to every 7000. Considering that the tables were the result of an original calculation, and that many of the errors were caused by the slipping of figures after the proofs had left the author's hands, the great accuracy of the table must be admitted by all. Two trivial errors may be noted; viz., Chilias 28 should be Chilias 38 in the first column of the page beginning with the number 37051, and the number in the corner of the page beginning with the number 66001 should be 66151 instead of 66251. In the course of the examination of the numbers, 49 were marked as containing figures so imperfectly printed, that they might lead to error; on referring to another copy, this was the case with but 25 of these; viz., 576, 4576, 7106, 16826, 19650, 21286, 24077, 30420, 31176, 31226, 33326, 37088, 41426, 45876, 61226, 61526, 66876, 66896, 81026, 83864, 96903, 97318, 97326, 97328, and 98280. Of course, in many copies these may be printed quite legibly, but the occurrence of imperfections in them in two copies renders it probable that they

may be similarly imperfect in others. It might also be advisable in any copy to ascertain that the slip of paper, previously noticed, containing the numbers 4776–4780, had not come off and been lost; but in point of fact the only part of the table ever used is that for the numbers 10,000 to 100,000, for which the characteristic is 4. It may be remarked that several of the alterations in Maskelyne's copy are made in the text, and these at first sight might give the impression that all the copies were not printed from the same types; such, however, is not the case.

The increase of the last figure in tables, when the succeeding figures are greater than 500 . . ., seems to deserve more attention than it has received. Errata, such as some noticed in this communication, where the succeeding figures are 499 . . ., are by no means uncommon; and it appears that the discoverers of them imagine they are doing some service by noting them. Take, for example, one of the cases in this note: the figures starting from the tenth are 5 49998 . . .; if we take 5 as the tenth figure, the error is 49998 . . ., if 6, the error is 50002, differing by 00004. Now, as our table only professes to give 10 places correctly (regard being paid to the magnitude of the figure in the eleventh place)! a difference in the fifteenth place does not come in question at all: 5 and 6 are both equally correct; they only differ by quantities, which throughout all the rest of the table we agree to neglect. It is a matter of regret that all such valueless refinements are not avoided by the author always explaining the exact convention on which the last figure is increased. A very convenient arrangement would be to understand that when x figures of a number were tabulated, the error was less than 6 in the next figure; or, if the calculator wished to be more accurate, 5 6 in the next two figures. To obtain a table of x figures, it is usual to calculate $x + 1$, or $x + 2$ figures, and the inconvenience of extending the calculation further in the particular case when the next figure is 5, or the next two 50, is, in many cases, excessive, and as the result is of no additional value when obtained, a figure "wrong" under these circumstances ought not to be styled an error. Probably a good many of Lefort's errata are of this class. Babbage, in the introduction to his well-known table of seven-figure logarithms, states, that in ninety-three instances the next three figures in Vega were 500, and that in all these cases the logarithms were carried to more than ten places to determine whether the figures were really 500 . . . or 499 . . ., and decide whether the least figure was to be increased or not. This appears to me to have been quite needless. It sets up an unnecessary and artificial standard of accuracy for the numbers whose seventh, eighth, and ninth figures happen to be 4,9,9 or 5,0,0. To the user of a table of seven-figure logarithms it is a matter of really no importance whether his error is 499 or 501; he is content to make an error of 5, and an additional error of ± 001 is of no consequence.

It is not a little remarkable that the most accessible table of

ten-figure logarithms we possess should have been published nearly 250 years ago. This places their use out of the power of all who have not access to an important library. In mathematical computations, ten-figure logarithms are very often wanted, even when only 5 or 6, or even a less number of figures of the ultimate function are intended to be tabulated. Most functions are expansible in an ascending (convergent) series of the form $A_0 + A_1 x + A_2 x^2 + \ldots$, and a descending (semi-convergent) series of the form $\frac{B_1}{x} - \frac{B_2}{x^2} + \frac{B_3}{x^3} - \ldots$ The former is usually very convenient for values of x less than unity, and moderately so till $x = 3$ or 4 perhaps, while the latter may, from the convergent portion, give the required number of places from, say, $x = 12$ or 20; but between the two practicable ranges of the two series there is usually a gap, and it is in cases such as these for the calculation of the intermediate values (if there is no continued fraction available for the purpose) that ten-figure logarithms are of the greatest use.

The title of Vega's work is *Thesaurus Logarithmorum completus ex Arithmeticâ logarithmicâ et Trigonometriâ artificiali Adrian Vlacq . . . a Georgio Vega. Leipsiæ*, 1794 (folio). The whole work contains 684 pp. of tables, of which the logarithms of numbers from unity to 100,100 occupy 305 pp. The arrangement is similar to that generally in use for seven-figure tables, and a great saving of room is effected: this advantage, however, is neutralised in the present work for the computer who only requires the logarithms of numbers by the other tables that are added. It is to be inferred from the preface that the publication of the tables was due to the great scarcity and expense of Vlacq's works; but, at all events in England, Vega's work is more scarce now than the latter. Many abridgments of Vega have been published.

Mention must be made of a work published last year; viz. *Tables de Logarithmes vulgaires à dix decimales, construites d'après un nouveau mode par S. Pineto . . . S. Petersbourg*, 1871, which by a new and ingenious arrangement in effect gives a complete table of ten-figure logarithms in 56 octavo pages. It does not seem quite so convenient as the older tables, and I imagine any one who can obtain the latter will prefer to use them.

Mr. Sang, of Edinburgh, has announced the intended publication of a nine-figure table of logarithms of numbers from unity to a million, which will be of great value to mathematical calculators; but, even with this work, the additional figure in Vlacq will prevent his table from being superseded entirely.

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Note on the Discovery of Saturn's Second Satellite.
By Richard A. Proctor, B.A.

In the obituary notice of Sir John Herschel which appeared in the *Daily News* for Saturday, May 13, 1871, the following passages occur. "It was long supposed that by the use of the great 40-feet telescope, and in the first moment, Sir. W. Herschel had discovered the sixth satellite of *Saturn*" (the second according to our present system of nomenclature); "and Sir John Herschel himself believed it till a controversy in 1843 occasioned a reference to a private journal of his father's, by which it appeared that it was the 20-feet telescope which was the instrument of the discovery." "We have referred to a controversy about the 40-feet telescope in 1843. At the meeting of the British Association at Cork that year, Dr. Robinson paid his tribute of admiration to Lord Rosse's telescope, and in the course of his remarks, said that however honourable the great Herschel telescope was to the philosopher and to the king of that day, it must be regarded as a failure, if judged by what it had effected. Sir John Herschel contested this assertion, fell into the great mistake of attributing to this instrument a discovery made with another, acknowledged his error, but continued his plea, &c."

Without attributing to this matter (really quite insignificant) the importance attached to it by the author of the above-mentioned obituary, I yet consider that as the point has been raised it is desirable that it should be rightly settled. I venture therefore to call attention to some evidence which seems to show that the belief accepted before 1843, was after all correct. It may be that those who remember the controversy then raised can correct me; but so far as the evidence cited in the obituary notice is concerned, I thing the rebutting evidence I shall bring forward is sufficiently convincing.

It appears that reference was made to a private journal of Sir W. Herschel's. Now I would venture to point out that a private journal would be much more likely to contain a faulty record than a public announcement. For an announcement communicated to a learned society would be carefully worded even as first written, and it would be revised (perhaps more than once) before being passed for press. Whereas we all know that a record may be hastily and carelessly written in a private journal. So that comparing directly the evidence of the private journal with Herschel's account at page 10, part I., of the *Phil. Trans.*, for 1790, I should be disposed to attach much more weight to the latter. It will be known to all who read this that in the account here referred to Herschel distinctly ascribes the discovery of *Enceladus* to his 40-feet telescope. But as this announcement was made some time after the discovery there may be some who will regard it as less likely to be correct than a record made at the time in a private journal. There is confirmatory evidence, however, which so far as I am aware has not been noticed in any

of our astronomical treatises. At the close of a catalogue of 1000 new nebulæ and clusters of stars, in the *Philosophical Transactions* for 1789, Part II., page 255, a postscript of seven lines occurs, which runs as follows: "The planet *Saturn* has a sixth satellite revolving round it in about thirty-two hours, forty-eight minutes. Its orbit lies exactly in the plane of the ring, and within that of the first satellite. An account of its discovery with the 40-feet reflector, and a more accurate determination of its revolution and distance from the planet will be presented to the Royal Society at their next meeting."

The paper to which this postscript is added was read June 11, 1789; but the satellite was not discovered until August 28, 1789. Doubtless the postscript was inserted by Herschel within a few days of the discovery of the satellite. It is at any rate the first published notice of the discovery; and its agreement with the full account published in the *Philosophical Transactions* for 1790, Part I., is very significant.

To this may be added the circumstance, that Struve, to whom Sir John Herschel presented his father's complete series of papers, with manuscript annotations by Sir W. Herschel, goes out of his way, so to speak (since he is writing about the stars) to mention that "le grand télescope de 40 pieds, achevé en 1789, conduisit immédiatement à la découverte du 6^me^ satellite de *Saturne*." *Etudes d'Astronomie Stellaire*, note 59.

When this paper was read I mentioned that it was my wish to elicit further information on the subject. Mr. Lassell thereupon pointed out that there had been a discussion in the columns of the *Athenæum* in which Mr. Robinson had, as he believed, gone far to establish his case. Mr. Lassell also mentioned that Sir W. Herschel had used his 40-feet telescope so far back as 1787, and had seen the second satellite with the 20-feet telescope in the same year.

Dr. Huggins and Mr. Lassell have since very kindly supplied me with further notes from the *Philosophical Transactions*.* These notes, combined with those I have supplied above, seem to give a complete explanation of the whole matter. They may be arranged as follows:—

First, as to the construction of the 40-feet telescope, Herschel says in the *Phil. Trans.* for 1795 (p. 350), "I had my first view through it on Feb. 19, 1787; but I do not date completing the instrument until later. . . . . I continued to work upon it till August 27, 1789. . . . . On August 28, 1789, having brought the telescope to the parallel of *Saturn* I discovered a sixth satellite of that planet."

Secondly, as to the observation of the satellite in 1787, Herschel says, in vol. lxxx. of the *Phil. Trans.* (his paper is dated

* I should mention that I have not convenient access at present to other volumes than those containing Herschel's researches into the fixed stars.

November, 1789), "I should certainly have been able to announce its existence as early as August 19, 1787, when at 22^h 18^m 56^s, I saw and marked it down as probably a sixth satellite, then about 12° past its greatest preceding elongation. . . . In the year 1788 very little could be done towards a discovery as my 20-feet speculum was so tarnished by zenith sweeps that I could hardly see the *Georgian* satellites. In hopes of great success with my 40-feet speculum I deferred the attack on *Saturn* till that should be finished, and having taken an early opportunity of directing it to *Saturn*, the very first moment I saw the planet, which was the 28th of last August, I was presented with a view of six of its satellites in such situations, and so bright as rendered it impossible to mistake or not to see them; and also on the 17th Sept. I detected the seventh satellite when at its greatest preceding elongation." In ascertaining the period of the sixth satellite, Herschel states that he used the 19th August, 1787, as a starting-point. Later he states that the seventh satellite "appears in the 40-feet, no bigger than a very small lucid point," yet he says, "I see it very well with the 20-feet reflector, to which the exquisite figure of the speculum not a little contributes." This is the account referred to in the former part of this paper.

It seems demonstrated then that though the satellite was seen in 1787 (as Hind also mentions), with the 20-feet telescope, it was not discovered in any proper sense of the word until August 28th, 1789, the instrument of the discovery being the 40-feet telescope. If the observation of 1787 is to be regarded as the discovery of the satellite, then by parity of reasoning Herschel did not discover *Uranus* nor did Galle observationally discover *Neptune*.

Note on the Densities of Jupiter's Satellites.

By Richard A. Proctor, B.A.

Incorrect values of the densities of *Jupiter's* satellites have somehow found their way into our text-books of astronomy, and have been repeated from one to another. They have led to erroneous assumptions respecting the condition of these bodies.

Thus, in Lardner's *Handbook*, we find the following table:—

Satellite.	Mass, that of Jupiter = 1.	Mass, that of Earth = 1.	Density, that of Earth = 1.	Density, that of Water = 1.
I	0·0000173	0·00520	0·02016	0·1143
II	0·0000232	0·00698	0·03015	0·1710
III	0·0000885	0·02663	0·06984	0·3960
IV	0·0000427	0·01285	0·03925	0·2225

It is strange that, though accepting different values of the satellites' diameters, Mr. Chambers, in his *Descriptive Astronomy*, gives the same values for the densities, only omitting the last decimal figure.

Whence these values were originally derived I cannot say. They are unquestionably incorrect, and are, in fact, not even near the true values. This is easily shown in any given case. Thus take the second satellite, whose diameter is a little over 2000 miles, or about $\frac{1}{40}$th part of *Jupiter's* mean diameter. Then, if of equal density with *Jupiter*, its mass would be about $\frac{1}{64000}$th part of *Jupiter's*, or would be represented by 0·0000156. But Laplace's estimate of the mass of this satellite is 0·0000232, or more than half as great again as that resulting from a density only equalling *Jupiter's*. Hence the satellite's density is more than half as great again as *Jupiter's*. But *Jupiter's* density is represented by 0·24 if the Earth's is taken as unity; and by 1·36 if the density of water is taken as unity. Hence this satellite's density would be represented by more than 0·36, if Earth's equal 1, and by more than 2·04 if the density of water = 1.

So much to show that the values tabulated by Lardner and Chambers are erroneous whencesoever obtained.

The following values of the densities of the several satellites have been obtained by combining Laplace's estimates of the mass, with the values of the diameters given in the second column:—

Satellite.	Diameter in Miles.	Density, Earth as 1.	Density, Water as 1.
I	2352	0·198	1·148
II	2099	0·374	2·167
III	3436	0·325	1·883
IV	2926	0·253	1·468

It will be observed that all the satellites except the first thus appear to have a greater mean density than *Jupiter*. Probably their real densities are greater than those here tabulated, since irradiation would increase their apparent diameters.

Note on the Orbit of the Double Star Castor.
By J. M. Wilson, Esq.

The orbit of *Castor* appears to be hyperbolic, a form of orbit which, as far as I am aware, has not been shown to exist in the case of any binary system.* Mr. Gledhill was good enough to furnish me with a list of measures of *Castor*, extending from A.D. 1740 to the present time, and on charting these on a table of

* [It may be interesting to compare with Mr. Wilson's hyperbolic orbit the elliptic orbit deduced by Mr. Hind from all the observations with which he was acquainted, ranging over the period from 1718 to 1845. In the *Monthly Notices* for December 1845, Mr. Hind remarks that "the elements are entirely different from those previously computed by Sir John Herschel and M. Mädler; and this difference is materially owing to the great influence exerted by recent measures at Mr. Bishop's observatory, by Mr. Dawes." The results "are as follows" (we quote from a note by Mr. Dawes in vol. xxxv. of our

engraved squares, and drawing and correcting the interpolating curve according to Herschel's method, I obtain the following eleven points for the determination of the orbit:—

θ.	*t.*	*r.*
340	1740·46	11450
330	52·44	10588
320	63·14	10188
310	73·34	10065
300	83·49	10119
290	93·89	10320
280	1804·89	10700
270	16·89	11256
260	30·39	12054
250	46·45	13254
240	66·49	15343

When the points are laid down they lie nearly on a hyperbola of eccentricity 2·2. The real hyperbola may be also shown by a graphical construction to have an eccentricity = 3·16, and its

Memoirs), "those given by Capt. Jacob at about the same date being added for comparison:—

	Mr. Hind in 1845.	Capt. Jacob in 1846.
Perihelion Passage	1699·26	1703·30
Projected place of Perihelion	° 8 15	° 10 0
Node	11 24	10 0
Angle between Perihelion and Node on Orbit	356 22	0 0
Inclination	43 14	43 17
Eccentricity	0·2405	0·300
Mean Annual Period	− 34'·163	− 33'·072
Semi-axis Major	6"·300	6"·30
Period	632·27 yrs.	653·1 yrs.

Capt. Jacob considered his results as only a rough approximation. Remembering that Sir John Herschel had obtained the period 253 years, while Smyth had obtained 240 years, we recognise the influence of recent observations, or rather of the progress of the motion of the component stars, in pointing to an increase of the orbit's extension. Mr. Wilson's result involves a somewhat startling advance in the same direction.

It may be added that the observations of Bradley and Pond give for the position-angle towards the close of the year 1720, 355° 53'.

The following measures are given by Dembowski in a recent number of the *Astronomische Nachrichten*:—

	Position-angle.	Distance.
	°	"
1866·02	241·07	5·384
1870·237	240·2	5·30
·327	239·5	5·64
·349	239·6	5·41
·68	239·34	5·488
1871·185	238·1	5·50
·324	239·3	5·59

It is worthy of notice that the study of this fine double star first impressed on Sir W. Herschel's mind "a full conviction," to use his son's words, "of the reality of his long-cherished views on the subject of the binary stars."—Ed.

line of nodes nearly coincides with the axis major. I hope to find time in the summer to examine the orbit by analytical methods.

The arc at present described is not sufficient to allow of a reliable graphical solution.

If my orbit is correct, the angle of position will decrease towards the limit of 188°. It is now about 238°·12, according to the observations of Mr. Seabroke and myself, and a little less than this by the interpolating curve, about 237·85. Perhaps some observers would communicate their measures during the past four or five years.

It would seem that *Castor* is a difficult star to measure. Some of the observations taken about the year 1865 especially must be in error by at least 3°, even on the mean of several nights.

On the Nutoscope, an Apparatus for showing graphically the Phenomena of the Earth's Motion called Precession and Nutation. By Prof. Ch. V. Zenger.

(*Communicated by John Browning, Esq.*)

In the case of a rapidly-revolving solid, as the Earth's, round its axis, two things may take place, according as the mass of the solid body is or is not uniformly distributed round the axis of rotation. In the first case the axis will steadily hold its position during the rotation, as is seen by rotating the top of a gyroscope. But if the motion of the rotating solid body is disturbed by a shock or overweight acting on one side, the axis of rotation will describe a cone and its apex a circle. This effect is produced by the attraction of the Sun and Moon on the Earth, conceived as a sphere rotating round its axis. The equatorial plane changes then periodically its points of intersection with the plane of the ecliptic, which we call the precession of the nodes.

In the second case, if the mass of the gyroscope is not equally distributed round the axis, and even this unequal distribution becomes changeable, which is practically done by putting on the axis of the rotating gyroscope a circular plate, with an excentric hole fitting loosely to the axis, the motion becomes more complicated, and the apex of the axis gets a double motion, a circular by the overweight acting as disturbing force on one side, and an elliptical produced by the change of position of that overweight, for the friction of the hole on the axis produces a retardation of the velocity of rotation of the circular plate. There will be described, as fig. 1 shows, a circle with a small ellipse revolving on its periphery, the centre of the ellipse being placed on it. The angle of the cone described thus will be changing periodically, and to a small extension, exactly as the complicated phenomenon of the earth's precession and nutation is due to the changing attraction by the Sun and Moon at

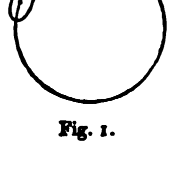
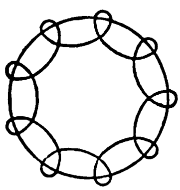


Fig. 1.

different distances and positions on the flattened or spheroidical earth-ball. The apex of the gyroscope in that condition represents, therefore, exactly the same kind of motion as that produced by the cumulated action of Sun and Moon on the rotating Earth; and this apparatus may, therefore, serve to show in a very plain and instructive manner the complicated astronomical phenomena of precession and nutation.

This is done by the apparatus that I call a Nutoscope. A very carefully-worked top of the known form is placed on a brass stand with a small spherical hole on its upper part, in which the sharply pointed axis of the rotating top is placed. The other end of its axis bears also a sharp point, that is slightly pressed on a blackened paper held by the hand in a quite horizontal position. It requires some practice to get a good drawing of the precessional and nutational curves by hand, and so I disposed the apparatus in such a way as to allow, by lowering with a micrometrical screw, a plate of paper fastened tightly on a brass ring moving along a cylindrical stand of brass near and perpendicularly to the axis of the top in its upright position. Another ring is fastened, with its plane parallel to the plane of the top in its upright position, the intersection of both planes giving the position of the nodes, the circular disk of the top representing the Earth's equator, and the plane of the ring the plane of the ecliptic. Giving the axis of the top by a fastened overweight an inclined position, the cone is described and the precession of the nodes shown on the periphery of the ring, and the apex of the top's axis describes on the blackened paper a circle.

Putting an additional changeable overweight, viz., the circular disk with the excentric hole on the top, the apex describes beautiful curved lines produced by the compound precessional and



nutational motion, as shown in the adjoining diagram. The greater the weight of the circular disk, and the slower the rotation becomes, the larger become the elliptic motion and the more distended are the curved lines. The same effect is produced by the friction of the apex of the top axis, which can be conceived as a changeable disturbing force: it is, therefore, difficult to get a top that would not give those pretty curves of nutation of its axis.

A great variety of designs of these curves may be got by changing the form and weight of the disk, the ellipses may be got so narrow that they become only visible by aid of a microscope. The appearance of these curves may become in some degree a means to account for the equal distribution of the mass in such apparatus as gyroscopes, tops, &c.

If the velocity of rotation is rapidly diminishing by the resistance as friction, &c., the top's axis describes with a fastened overweight a spiral instead of a circle, and with a changeable overweight there is described a spiral line with an ellipse revolving with its centre on the spiral line.

Note on ξ Ursæ Majoris. By J. M. Wilson, Esq.

The subjoined measures of this double star were taken by Mr. Seabroke and myself:—

t.	θ.	No. of Readings.	r.	No. of Readings.	Observers.
	°		″		
1871·44	43·9	(4)	..	..	W
1872·13	23·15	(7)	1·04	1	W & S
·18	22·95	(14)	·97	5	W & S
·20	23·3	(4)	·91	2	W & S
·21	23·0	(6)	1·19	(2)	W
·24	20·3	(3)	..	..	W
·35	19·7	(4)	1·18	2	W

It is near its periastron, as I pointed out in the *Astronomical Register* of February, and its angular motion is very rapid. I do not know what weight to attach to this series of observations as they do not agree very well with those given by Mr. Knott, a very much more experienced observer. His measures given in the *Monthly Notices* for March are:—

t.	θ.	r.
1872·028	31·6	1·092
·088	29·69	1·111
·138	28·15	1 052

It deserves careful watching at present. Its period is now shown to be between 61·0 and 61·2 of a year.

Note on ζ Cancri. By J. M. Wilson, Esq.

This very interesting binary of short period has recently passed through its periastron; and I annex my observations of it, which may be valuable, as it was not observed at all at its last periastron in the beginning of the century:—

t.	θ.	r.
1872·18	168·6	0″·78
·19	169·3	0″·63
·25	165·25	..

All the measures were taken with Alvan Clark's 8¼-inch refractor and a parallel wire-micrometer.

Mr. Christie kindly observed the star at my request, and allows me to communicate his measures made with the Greenwich Equatoreal:—

t.	θ	r.
1872·33	157·6	0″·4±.

Stars very tremulous.

Temple Observatory, Rugby, May 9, 1872.

Elements of Minor Planet (119).* By M. Péchule.

From observations made at Ann Arbor on April 3, at Paris on April 11, at Lund on April 19, and at Hamburg on April 28, I have calculated the following elements of this planet :—

Epoch 1872, April 28·5, Berlin M.T.

$$\begin{aligned} M &= 164^\circ\ 21'\ 24'' \\ \pi &= 36\ 34\ 33 \\ \Omega &= 203\ 39\ 38 \\ \iota &= 6\ 30\ 30 \\ \phi &= 8\ 3\ 11 \\ \mu &= 862''\cdot08 \\ \log a &= 0\cdot40964 \end{aligned}$$

May 2, 1872.

Observations of Planet (120)†. By Dr. C. H. F. Peters.

On the 11th inst. I found a small planet 11·5 magnitude, of which I take the pleasure to communicate the following positions, all that it has been possible to obtain during the unfavourable weather :—

1872.	H. C. M.T. h m s	α (120) h m s	δ (120) ° ′ ″	
April 11	14	12 0 5	−4 59 0	Approximate.
16	13 37 29	11 56 52·42	−4 44 39·1	12 comp. } with Schjellerup 4348.
17	11 7 16	11 56 20·96	−4 42 15·8	10 ,, }
19	11 52 8	11 55 13·69	−4 37 1·6	10 ,, with Lalande 22646.

Litchfield Observatory, Hamilton College,
Clinton, New York, 22 *April*, 1872.

Eclipse of Jupiter's Third Satellite observed on April 11*th*, 1872, *at Forest Lodge, Maresfield.* By Capt. W. Noble.

The satellite which, as usual, faded out very gradually did not fairly disappear until $10^h\ 2^m\ 24^s$ L.S.T. = $8^h\ 40^m\ 56^s{\cdot}4$ L.M.T. The predicted Greenwich mean time of disappearance, in the *Nautical Almanac*, was $8^h\ 38^m\ 19^s{\cdot}0$, so that I *ought* to have lost sight of it at $8^h\ 38^m\ 36^s{\cdot}5$ L.M.T. (I assume my longitude to be $17^s{\cdot}5$ E. of Greenwich), or $2^m\ 19^s{\cdot}9$ sooner than I did. This discrepancy, however, is much smaller than usual for one of the outer satellites. I employed a power of 154, adjusted on the planet, on my 4·2 inch Ross Equatoreal.

* [This planet was re-discovered by M. Paul Henry, at Paris, on April 9. The actual discovery must be credited to Prof. Watson, of Ann Arbor. Ed.]

† This planet is the same as that discovered by M. Borelly, at Marseiles, on April 10.

Occultations of Stars by the Moon, observed April 18, 1872, *at Maresfield.* By Capt. W. Noble.

Occultation of B.A.C. 3579 *by the Moon.*

The star disappeared instantaneously at the Moon's dark limb

At $9^h\ 48^m\ 37^s\!\cdot\!2$ L.S.T. = $7^h\ 59^m\ 40^s\!\cdot\!8$ L.M.T.,

and reappeared at the bright limb

At $11^h\ 6^m\ 19^s\!\cdot\!5 \pm 1^s$ L.S.T. = $9^h\ 17^m\ 10^s\!\cdot\!4$ L.M.T.

Afterwards I observed the

Occultation of i Leonis.

The star disappeared instantaneously at the dark limb

At $12^h\ 2^m\ 46^s\!\cdot\!0$ L.S.T. = $10^h\ 13^m\ 27^s\!\cdot\!6$ L.M.T.

Massive cumuli covered the Moon at the time of reappearance.

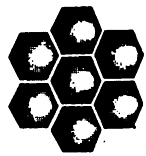
Power in each case 255, adjusted on the first-named star.

On the Value of the Stereoscope as applied to the Examination of Eclipse Photographs. By A. Cowper Ranyard, Esq.

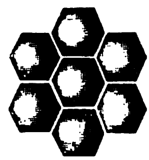
At the last meeting of the Society it was suggested, that by combining in the stereoscope two photographic pictures of the corona, the one taken at the beginning of totality and the other towards the end, good and reliable evidence may be obtained that the corona is a solar appendage. It was further asserted, that in such a combination, if properly made, the dark body of the Moon and the corona both seem to stand out as solid bodies. If such stereoscopic appearances may be relied upon, their value in proving the solar nature of the corona, as well as in showing the way in which the streamers are arranged upon the solar surface cannot be too much exaggerated; for without them we are in possession of the mere projections upon a plane of an arrangement of details which curve and interlace with one another in three dimensions.

It becomes, therefore, of the greatest importance for us to examine whether, by such a combination, it is possible to obtain any true stereoscopic effect — by which I mean an effect which is produced by the parallax of nearer objects upon those further from the observer.

It is well known that suitable shading will influence our minds in that compound process often called the judgment of the eye; and the sloping together of lines in apparent perspective is continually used by artists whose business it is to make space of two dimensions appear as if it was space of three.



The way in which we may be deceived by suitable shading is very well illustrated by the annexed drawing, which is made from a photograph taken a few years ago by Mr. Beck. The mere alteration of illumination caused by turning the drawing round will make the parts of the picture which in the one instance appear to be convex to appear as if they were concave.



It is also certain that our judgment is much less acute and discriminating when we view a picture through a lens than when it is looked at directly with the naked eye; for this purpose, single lenses are used to give apparent relief to photographic groups of flowers and other objects.

We cannot, therefore, be too careful in examining the real causes of any mental impressions that we may receive when looking at photographs through the medium of lenses, before we proceed to rely upon them as affording good proof of horizontal parallax between the two superposed pictures.

Let us now, therefore, examine what chance we have of finding such a difference caused by parallax in two corona photographs, taken, the one at the beginning and the other towards the end of totality. If there were four minutes of time between the pictures—which is, indeed, a longer period than has elapsed between any of the corona photographs that have yet been taken—the Moon in that time would have moved through about two minutes of arc, covering up on the one side and discovering on the other the solar prominences, as was clearly shown in 1860; and so far as the prominences are concerned we have a true effect of parallax, and reliable proof that the prominences are behind the Moon.

But when we come to the parts of the corona in which there is no covering and uncovering, our changes consist in the difference of distance from the Moon's limb of any details of the corona which may be detected upon a careful examination of the two photographs. By details I mean such clearly recognisable points or curve lines as exist *within* the corona—the outer boundary or contour of the coronal light depending evidently upon the amount of exposure or the different development of the two photographs.

Any such increase of distance from the Moon's limb on the one side, accompanied by a decrease of distance on the other side, may be regarded as sufficient proof that the shifting details are unconnected with the Moon, or, at all events, are not carried along with it in its motion; though it still remains for us to show from other considerations whether such details lie in front of, about, or behind the Moon. But as regards the nebulous and hazy portions of the corona we can have no such measurable and reliable proof, and we are left only to the natural inference of

our minds that the nebulous haze is probably connected with the details which it surrounds.

It is, again, the inference of the mind which, upon looking into the stereoscope, associates those details of the corona which are not covered and uncovered by the Moon with those details which are so covered up and uncovered, or rather with the solar prominences, for there is very little detail of the corona proper to be seen at points so near to the Moon's limb.

If two similar photographs of the corona, copied from the same negative, be placed side by side in the stereoscope, the same effect of the Moon standing out in front of the corona is not produced.

We may therefore regard the stereoscope as a rough and ready means of detecting horizontal parallax between two photographs, though a much more satisfactory determination of the existence and amount of such parallax may be arrived at by a careful measurement of the photographs themselves.

Secondly, as to what may be learnt with regard to the disposition of the rays of the corona from any appearances of relief that may be detected with the stereoscope.

During four minutes of totality, our Earth would move through an angle, with respect to the Sun, of somewhat less than ten seconds of arc; while the Sun would turn upon his axis (carrying, we may conclude, all the detail of his coronal rays with him) through an angle of about two minutes of arc. This would leave a total alteration in the position that the arrangement of coronal rays would present to us of about one minute fifty seconds of arc, an angle too small to give appreciable relief in the stereoscope. That such an angle is far too small may be seen from the consideration that the maximum possible shift given by such a revolution to the end of a ray extending to the distance of the Sun's radius from the photosphere would not subtend an angle of one second, as viewed from the Earth, and the shift of such rays as are visible to us would be much less.

We must therefore reject all such appearances of solidity and relief amongst the rays of the corona as spurious effects produced by shading or some of the other unknown causes which are apt to deceive the mind.

On the other hand, the stereoscope is undoubtedly useful as affording a ready means of superposing, and thus showing the absolute identity as to form of the details of the corona at the beginning and end of totality.

The Tables of Jupiter's Satellites. By J. Maguire, Esq.

The proposal of the Astronomer Royal, published in the January Number of the *Monthly Notices*, viz., that one observatory should be permanently devoted to the observation of the phenomena of *Jupiter's* satellites, has undoubtedly a pre-

ferential claim to the consideration of the Royal Astronomical Society, and I think I may safely add that it has an interest for many outside the Society, who are in the habit of using a telescope. As one standing near the threshold of the Temple of Science, but not, he fears, having in his possession the password which confers the privilege of entering and mingling with the votaries within, I am glad to find that the time has at last come when the judgment and influence of the Society are likely to be successfully employed in establishing such an observatory.

Of the want of a public observatory other than meridional, there cannot I think be a question. Col. Strange proclaims the want and points to the quarter from which support should come. It may be observed that whenever a new Ordnance map of these islands is declared upon authority to be wanted, theodolites and other instruments are very soon forthcoming for the work. And who will venture to say that England has spent so much money on theodolites and Armstrong guns that she cannot afford to set up a few equatoreals for the purpose of observing the satellites of *Jupiter* in order to construct new tables which would give to several pages of the *Nautical Almanac* a value which they do not now possess? Any idea of this kind is instantly put to flight by remembering the various liberal grants recently made by her Majesty's Treasury for the observation of the phenomena of solar eclipses, and the satisfactory provision made for the observation of the transit of *Venus* in 1874.

It is not pleasant then, to observe the rather desponding tone of this passage which I quote from the paper of the Astronomer Royal: "I see no prospect of obtaining a sufficient number of observations unless some observer would consent to give his entire observing energies to these observations."

Upon whom may I ask has fallen the mantle of the observer of *Gamma Virginis?*

Norwich, May, 1872.

On Astronomical Units. By C. C. De Crespigny.

In treatises on astronomical subjects I have observed that in English works, diameters and distances are expressed in British statute miles of 1760 yards each, in French works in kilometers, and I presume that in German and other works they are expressed in terms of the standard measure of length of the country in whose language the treatise may be written: also that in England we acknowledge another mile of 2026 yards, the sixtieth part of a degree of the equator, which, as it is also acknowledged in other countries, may be considered to have a greater right to be accounted a mile than the statute mile; at least when speaking on astronomical subjects. I have also read the remark in treatises above alluded to sometimes appended to a statement of the distance of a star

from the Sun or of a planet from the Sun, that the human mind fails to form any appreciation of such an enormous distance. As it is one of the pleasing duties of any particular science to eliminate doubts and want of appreciation from the minds of students, and as it is difficult for students of one country to form a just appreciation of weights and measures when expressed in terms used in countries not their own, and as it would be generally desirable to have uniformity in expressions of astronomical distances as there already is in those of masses and densities, I venture to make the following suggestion in the hope it may be considered worthy of thought by some member of your Society. It is simply this, that ingenuous youth should first be taught the value of the 60th part of a degree and call that one mile. Then call the diameter of the Earth 6875 miles instead of 8000, as at present. From that point let his mind be led to contemplate the size of the Sun, the diameter of which he may be taught to express either in miles or in Earth's diameters. Then, instead of telling him that the Earth is 92 or 93 millions of miles (British) from the Sun, and at the same time stultifying him by saying that the mind fails, &c.; let him simply be taught that the Earth is *only* 100 of the Sun's diameters from that body, and his mind will at once appreciate the fact that the Earth is in reality very near the Sun, and he will be able to grasp with ease the distances of the exterior planets expressed in the same terms. He might for his own amusement reduce these distances to British miles, and it is to be hoped that the interests of the British inch would not be endangered by this method.

In the same manner distances of fixed stars, wherever determined, might be expressed in terms of the diameter or radius of the Earth's orbit.

Sarawak, February 1872.

The Astronomer Royal has received from Mr. Stone the following communication, dated Royal Observatory, Cape of Good Hope, 1872, April 18 :—

"I have printed now 1856, 1857, and 1858, and have 1859 ready for press, and 1860 well in hands ; but I hope also to bring out the results for 1871 this year."

A Paper by Mr Brett, "On an Altazimuth Mounting for Telescopes, especially adapted for the use of Observers who have no permanent Observatory," with an illustration, will appear in the next number of the *Monthly Notices*.

MONTHLY NOTICES

OF THE

OYAL ASTRONOMICAL SOCIETY.

XXII. *June* 14, 1872. No. 8.

PROFESSOR CAYLEY, President, in the Chair.

Rev. Thomas Tordiffe, Workington ;
Rev. Wm. Falconer, Bushey ;
Rev. F. W. Hall Courtier, Clare College, Cambridge ;
·. Sheppard, Esq., Winchester ;
M. Williams, Esq., Woodside, Croydon ; and
V. Jackson, Esq., Tooting,

lloted for and duly elected Fellows of the Society.

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scopic Observations of the Zodiacal Light, in April 1872, *he Royal Observatory, Palermo.* By C. Piazzi-Smyth, 'onomer Royal for Scotland.

Introductory Account.

ing been required last winter to seek a warmer climate ıe, I endeavoured when away to utilize the opportunity ıting those observations on the zodiacal light, in which I pletely failed in obtaining any positive result through the ıring season of 1871, on the top of Calton Hill, Edinburgh. lure there arose from the smokiness and gas-illumined ere of a northern city, preventing so faint and low a fluence as the zodiacal manifestation is there, being ever recognised as such.

when fine weather broke at last upon me, on March 1, latitude 40° 30′ N., far away from gas and smoke, I ect to see something that evening, though I was hardly l for the full magnificence and vigour with which, from P.M., local mean time, the well-known lenticular form zodiacal light was seen cutting obliquely up amongst the

stars, and contrasting notably with everything else visible in the sky at the same time. Thereby the remembrances of thirty years ago, in S. Lat. 33°, were instantly brought back to me; the zodiacal light was still to my eye as it was then, several times brighter than the Milky Way, of regular mathematical figure and shading, and altogether one of the grand phenomena which an astronomer might worthily desire to probe the possible nature of. So also it continued to be seen, changing its place regularly among the stars with the Sun's motion, both in Right Ascension and Declination, through the months of March, April, and May, whenever absence of the Moon, twilight, and clouds, permitted a fair observation to be taken in any latitudes approaching to 38° or 40° north.

During the middle of the month of March, being stationary in Palermo for a time, I prepared my spectroscope, at the hotel window where I was staying, for acting on the zodiacal light as soon as the moonlight evenings should cease. But having in the meanwhile made the personal acquaintance of Signor Cacciatore, the Director, and Professor Tacchini, the chief observer in the Royal Observatory of Palermo as well as secretary to the Spectroscopic Society of Italy and editor of its transactions, they saw the apparatus on paying me a visit one day, and immediately proposed that I should institute all the intended observations at their Royal Observatory, whose elevated situation was far more appropriate for the purpose than any house in the city below. To this proposal, made in the most admirable spirit of fraternal accord in working for the promotion of knowledge, I was most happy to consent, and all the more because opposite conclusions in the problem were already in the field, and I wished no step in my process to be concealed or mysterious. So during the several days which must still intervene before the Moon should be completely eliminated from the early night sky, the apparatus was taken to the Palermitan Observatory, and tried occasionally on test-objects; while in frequent day visits I profited in solar spectroscopy by seeing Signor Tacchini's masterly handling of the 20-prismed instrument attached to the Palermitan equatoreal, comparing his daily drawings of red prominences with those furnished for the same dates by Padre Secchi at Rome, and Signor Lorenzoni at Padua; and generally in reading up all that Italian spectroscope astronomers had been doing for some time past, both on the aurora and zodiacal light, as well as on the Sun, in the papers, both printed and MS., of "the Spectroscopic Society of Italy," freely lent to me.

At last the first favourable evening arrived, the spectroscope was already in suitable position for its cosmic work at the Observatory; and on April 1, from 7.45 to 8.50 P.M. the zodiacal light, admirably and characteristically visible, was well worked by Signor Cacciatore and myself; again, on 3rd of April, during the same hours, by Signor Cacciatore, Signor Tacchini, and myself; and again, on the 5th of April, by Signor Cacciatore,

Signor Ricca, my wife, and myself. Each evening gave the same results, and every observer saw the same spectrum in the same manner, excepting only in some minute and unimportant residual differences of different eyes when tried on an instrumental appearance of the very last degree of faintness. And the spectrum which was thus found for the zodiacal light was a short continuous spectrum extending from near to W.L. 5550 to near W. L. 5000, and culminating as to its brightness in or about W. L. 5350, while it was shaded off indefinitely into darkness at either end.

In short, a spectrum this was, as different from that of the aurora when seen in the same instrument, "as night from day," according to the description of a lady who had seen the spectra of both these phenomena in the same instrument, on different occasions, and was invited to give her opinion in the Palermitan Observatory on our concluding zodiacal light evening of April 5—an opinion, too, in which I perfectly coincide; but then I am required as a man to give a reason for the opinion that was in me, and to show that no other was possible to any good head or clear eye. I must crave, therefore, sufficient space to set forth,—

1st. A description of the Edinburgh faint-light spectroscope.

2nd. Comparison of the new results, with older ones already published; and

3rd. Phosphorescence spectra, astronomical and terrestrial.

The Edinburgh Faint-light Spectroscope.

This instrument was built up by degrees at home in 1870 and 1871. I could make a better one now, and applied to the Royal Society before going out in February for a portion of the Government grant to construct such an apparatus specially for the zodiacal light; but as the Council refused to give me anything, I had no resource but to continue the employment of the old instrument, intended mostly for the aurora, but capable, fortunately, of proving some points in observation which are generally rather uncertain at night, and particularly with portable instruments in unusual situations. Thus, say that an observer looks (as I did when at sea on March 1, 1872) at the zodiacal light with a little hand spectroscope casually purchased, and sees something or other excessively faint near the middle of a perfectly dark field, what account can he give of it? The light is too weak to show any colour; there is nothing else visible in the darkness to fix the spectral place by, nor to prove the width of the slit, nor the focal sharpness of that slit, nor to show the amount of dispersion of the prism; yet each and all of these points should be fixed there and then, or nothing certain has been arrived at.

The above observation-difficulty was met in the larger

Edinburgh apparatus by employing invariably a reference spectrum formed out of the blue part of the flame of an alcohol lamp, very slightly tinged with sodium and lithium chlorides, but with all the upper yellow or continuous-spectrum giving part of the flame cut off. This sort of light, then, being brought in sideways, and passed by reflection through the lower part of the same slit and prisms used in examining the cosmic phenomenon, gave its own known spectrum in the lower part of the field, and enabled the observer, though in perfect darkness, to know, first of all, whether he was looking straight into the prism eyepiece; second, to test the focus of slit by the sharpness of the edges of the lithium and sodium lines; third, the breadth of the whole field by the visibility of all the five bands of the blue alcohol light from red to violet; fourth, the width of slit by the width of the above lines compared with their distance asunder; and fifth, the dispersion power of the prisms by the degree to which, with the finest slit and the best focus at that part of the spectrum, either the citron or the green band of the blue flame could be separated into its component fine lines; such state of the slit and focus being found also to be the best for seeing the Fraunhofer black lines by day in the same spectral region.

Now in the Edinburgh apparatus, when consisting optically of a slit, an achromatic collimating lens adjustible for focus, and a direct-vision prism-combination about four inches long, I could with such a "Fraunhofer-line slit," directed on the reference-lamp, only fancy that I saw the separate lines: *i. e.* the resolution of the *bands* was not clear, though it had commenced.

With a second similar prism, or with a little opera-glass used in addition, the resolution of the bands was certain, and their lines were seen separated from each other by about twice their own thickness; but these additional aids to dispersion effects could never be used with advantage on the zodiacal light by reason of the peculiar faintness of its spectrum.

Two similar alcohol lamps being simultaneously employed on a certain preliminary occasion, one was looked at direct with the telescope tube as a cosmic phenomenon, while the other was placed in the standard manner on one side of the tube as a reference lamp inside a dark lantern. The spectra of the two lamps were then seen in the prism eyepiece spread horizontally, one over the other, and every band and every line of the one was standing in exact vertical coincidence over those in the other, saving a very little ragged light along the base line of separation of the two spectra, but which was of no real moment. There could then be no reason, on substituting the zodiacal light for the one lamp at the end of the tube, why the zodiacal light's spectrum, if bright enough, should not be seen honestly and fairly standing over the lower spectrum derived from the side reference-lamp.

Our place of observation was unexceptionable. A window looking west out of the upper part of the Royal Observatory of

Palermo, and that observatory again standing on the summit of the Royal Palace, at 120 feet above the ground; that, too, a rising ground, looking far over the house-tops of the city, at a horizon formed of distant dark hills. A few lamps from a square far below did shine up at us, but a black cloth spread over the balcony cut off their rays; and every light was excluded from the apartment we were in, excepting only the reference-lamp of the spectroscope, and that was allowed merely to throw a small portion of its faint blue base of flame into a concealed part of the instrument. The slit of the spectroscope, too, was perfectly protected from stray side-light, for it was at the bottom, or inner end, of the blackened telescope tube of the apparatus—a tube originally intended to carry a large collecting object lens, but the use of which lens was found rather prejudicial than otherwise as to brightness on so broad a surface as that of the zodiacal light.

From that window, then, of the darkened room at the top of the palace, we looked out to the west, and there was the zodiacal light brilliant, so to speak, instantly recognised by all of us as being, as it should be, for the vernal equinox in Lat. 38° N., and vastly brighter than it ever could be seen in any very northern city as of 50° or 60° Lat. Upon that most visible zodiacal light, then, we directed the grand tube of the spectroscope, its slit being arranged narrow, or as most suitable to resolving the reference of blue flame bands into lines; or, as I may add from long Edinburgh experience, for seeing and identifying the one bright line formed by the peculiarly monochromatic light of ordinary green aurora. On looking into the eye-piece, there were all these bands of the reference spectrum with symptoms of linear resolution, together with the lithium and sodium lines thin, as representing the fine slit, ranged across the lower part of the field; but in the upper part of the field where the spectrum of the zodiacal light ought to have appeared, there was nothing. To make quite sure, the instrument, carrying the reference-lamp along with it, was moved slowly, first in azimuth and then in altitude, backwards and forwards across all the brightest parts of the zodiacal light; the reference-lamp, too, was dulled, and at last altogether excluded, but still nothing whatever was seen on looking with the prisms through the slit at the zodiacal light, which otherwise was not only abundantly bright to the naked eye, but also when viewed through the same prisms if used without any slit at all.

The only explanation, then, that is possible for the non-appearance of any spectral light, whether in lines, bands, or anything else, when the fine slit was used, is, that the light of the zodiacal manifestation is not, as has been sometimes asserted, monochromatic light of one definite refrangibility (in which case it could only give a line as bright as the full phenomenon, and as narrow as the slit), but it is of many various refrangibilities, and

spread thereby over so large a spectral range as to become weakened down to practical invisibility.

To ascertain over how great a spectral range the zodiacal light is spread, the slit was opened slowly until *something* was seen in the dark field, and then it was no line or lines, but a short portion of continuous spectrum that was indicated, becoming clearer, and rather larger too, when the slit was still further widened. Indeed we made the slit at last even extravagantly wide, or approximating in such breadth to the length of the said portion of continuous spectrum itself.

This portion, however, was never definite, never bounded by sharp upright parallel lines, as were the lines of lithium and sodium with that broad slit; but it was shaded off gradually into darkness at each end, and had its general maximum of light rather nearer the less refrangible end. Moreover, it was altogether so faint, as well as undefined and indefinable at the same time, that it could never be seen certainly in conjunction with the reference spectrum, the latter having to be excluded and then let in again, so that the comparison of the two might be made by memory assisting the eye.

These characteristic features of the zodiacal-light spectrum are represented in an accompanying plate in its spectrum pairs 1, 2, and 3, and are, I believe, substantially agreed to by Siguor Cacciatore and Professor Tacchini, as well as myself.

Comparison of the new results with older ones already published.

To prevent any needless delay, as happened to a similar case last year, in examining, weighing, and publishing these results of observation, I refrain from mentioning the *names* of the various leading spectroscopists of the day, by whom totally opposite conclusions from those now detailed have been arrived at; it is sufficient for the scientific discussion of the case as it exists in nature, to mention what these distinguished astronomers have concluded, and to remind that their decisions have hitherto been generally accepted by the public, and are considered to have proved, that the spectrum of the zodiacal light *is the same as that of the aurora*.

And what *is* the spectrum of the aurora like, according to these authorities?

The chief part of them say that it is for the ordinary pale aurora simply monochromatic, or consisting, with a fine slit, of a single and sharp definite greenish line near W. L. 5579; yet some amongst those authorities there are who stand out for a rather brilliant continuous spectrum also. These latter persons, however, I do not scruple to say, are in error. They were generally those upon whom, in Southern European latitudes, the splendid aurora of February 4 came so astoundingly and unprecedentedly

that they rushed out—all inexperienced—with chemical spectroscopes, telescopic-star spectroscopes, sun-prominence spectroscopes, and anything at hand, however inappropriate, and hazarded a spectroscopic shot at the unusual phenomenon, under circumstances exciting, fascinating, agitating, to the last degree; and never repeated since. I saw too, some time afterwards, also in a Southern university, how a grand continuous spectrum might have been incontinently obtained by the use of one of those instruments, even out of, or with the monochromatic green light of aurora, and from the red portion too on that unique occasion.

The party in question had a magnificent 4-prism chemical spectroscope, and desired very obligingly to show me the D lines in daylight from the blue sky, as seen through a window of the apparatus-room. The party laboured and laboured, but could get no lines at all, only an impure continuous spectrum. At last a friend of rare skill arrived, who discovered that the portion of light entering direct through the slit was swamped by the superabundance of the light entering slantingly *behind the slit* into the hole of vision in the side of the brass box covering the prisms. This was an effect which would not have occurred if the source of light had been small, but was thus destructively powerful when the source was of large area, as it was undoubtedly either with the said window, or still more with the aurora of Feb. 4, for that was on all sides of the heavens at the same moment.

What then, with auroral light pouring unexpectedly into the spectroscope by a circular hole 1·3 inches in diameter, the indiscreet use of lamps, and the insufficient avoidance of other lights on an *impromptu* occasion of promiscuous observing, there is no difficulty in explaining the false continuous spectra once ascribed by certain persons to the aurora, even when using a fine slit. Hence we may fall back in confidence on the accounts of the larger part of spectroscopists who say, and say truly, with the first of all auroral observers, M. Angstrom, that with a *fine slit* on an *ordinary aurora*, they see only one fine or thin greenish line.* So fine, sharp, and bright, indeed, is this line, or so undiluted by dispersion, that even on faint beams of aurora, not brighter than the zodiacal light, and with a fine slit, of course, I have myself used three times the dispersive power that that latter phenomenon would not bear even with a broad slit; and still, not only have I then seen the auroral line distinctly, but compared its place neatly against that of the second line in the citron band of the blue base of alcohol flame in W. L. 5579.†

* I am confining myself here to an ordinary, or greenish aurora, and a fine slit: what may be seen on a red and extraordinary aurora, and with a wide slit, I have already described in the *Edinburgh Astronomical Observations*, Vol. XIII.

† See Spectrum-pair 7 in the accompanying plate; also the plates in the XIIIth Volume of *Edinburgh Astronomical Observations*. I have since then learnt, by correspondence with Dr. W. M. Watts, and by reading his useful little book, *Index of Spectra*, that each of the fine lines which I describe in the

If, then, the true state of the case stands thus, that the ordinary auroral fine-slit spectrum is one thin fixed line, how can those persons be justified who assert that it is the same spectrum as that of the zodiacal light, viz., that very one which we have shown, I hope, incontestably, to be a most diluted and dispersed portion of continuous spectrum, broad, indefinitely shaded off at either end, and far too faint to be appreciated by the human eye when employing a narrow slit and prisms of a decent amount of dispersive power? My answer must stand thus:—

1. Some of the observers were in high latitudes, and had not eliminated their frequent auroral glows from their only occasionally supposed and most faint zodiacal light. They ought evidently to try their observations over again in parts of the world where there is less aurora and more zodiacal light.

2. None of the observers, whose accounts I have been able to read, employed any reference spectrum to prove either the focus, breadth of slit, dispersion, place in spectrum, or any other necessary condition under which they were observing: and there are documents against some of them, connected with their alleged places of the principal aurora line on Feb. 4, showing that they cannot safely dispense with such instrumental check and guide, even in a far easier case of observation than that of the zodiacal light.

3. The spectroscopes employed were generally little portable things of such small dispersion but very tall slits, as to make what was really a broad tract of continuous spectrum to look like a narrow band, or an ill-defined line; and this appearance being seen in an instrument whose focus may be rather uncertain at night, and which has on previous occasions shown the auroral line instrumentally blurred, is hastily declared to be a line also.

4. Other observers, again, who, with a broad slit, have seen the continuous spectrum of the zodiacal light, and have marked a fine bright line, like that of the aurora when seen with a fine slit, though in a different spectrum place, down the middle of it, have let their fancies run away with their judgment and have pictured that which is spectroscopically impossible, considering the large surface of the zodiacal light, no matter what the physical characteristics of that light may be, monochromatic, polychromatic, or anything else.

5. And, finally, those observers who have painted a very *brilliant* picture of the zodiacal light spectrum, whether continuous or linear, have failed in representing one most characteristic feature of the fact, viz. its faintness.

In short, though I am sorry to say it, none of the observers who have declared for the spectra of the zodiacal light and aurora

coloured bands of blue flame base, or acetylene, or carbon, breaks up, under higher dispersion, into a finer species of lines still; but *their* places do not seem to have been determined yet by any one; and his final wave-length numbers still therefore apply to my "lines," though to his bands; my "bands" being his groups.

being the same, have proved any of the necessary elements of their observing methods.

Phosphorescence Spectra, Astronomical.

Not only, however, have we to deal in the present day with the above too often published misleading equation between two grand physical apparitions, but there is another equation of the same order put forth by some of the same high authorities; viz., that on certain nights when "there is a general phosphorescence over the whole sky, the auroral line (W. L. 5579) may be detected in that light equally over it all."

Of course, if the aurora *itself* is spread on any occasion over the whole sky, its line must be spectroscopically recognisable everywhere; but phosphorescence is not aurora, and aurora is not phosphorescence; and that which some observers mysteriously call by this name "phosphorescence," and have never attached any peculiar spectrum to, is, in my humble apprehension, nothing but star-shine and night-light, similarly with sunshine and daylight. Starshine, of course, exists for every one who can see the principal stars; and the night-light of the background of the starry sky is just as certainly a fact, being made up, not only of the reflected light of those brighter stars on our own atmosphere, but of the direct light of myriads of smaller stars far too minute to be individually appreciated by the human eye.

And what sort of spectrum ought such star-shine and night-light to offer to the observer? Not a sharp, aurora-like line at all, but something, according to the manner and locality of its formation, very much like the spectrum of the last twilight of day. In fact, according to numerous observations on the Sicilian night sky, when free from any accusation of aurora, I found such night-shine to yield a short continuous spectrum; and that culminating, not at the aurora line place of W. L. 5579, but near W. L. 5350.

Now this place evidently corresponds within the limits of error of observation to that of the last residual portion of the continuous spectrum of stronger daylight or sunshine (with the eclipse corona line also, at W. L. 5322), modified slightly by the absorbing and reflecting powers of the atmosphere.* No Fraun-

* On the 6th of April, in Palermo, I made the following observations with the auroral spectroscope, but turned now upon the ordinary evening twilight, the slit being fine or proper for seeing Fraunhofer lines at midday. The alcohol reference-lamp was also employed to give further proof of the state of the slit and to indicate the spectral position of anything that might be seen in the twilight spectrum; which spectrum, in less than an hour, passed through all the following stages.

Stage 1. A continuous bright-coloured spectrum, showing the usual solar and telluric black Fraunhofer lines distinctly.

Stage 2. Continuous spectrum faintly coloured; Fraunhofer lines have now disappeared; but are replaced by darkish bands, probably formed of aggregation

hofer lines are seen in such star-shine spectrum for these reasons: 1st, the continuous spectrum is too faint to show any black lines upon it clearly; and 2nd, the slit used at the time of observation was too broad to have shown Fraunhofer lines even in noonday shine. Indeed to see the spectrum of general night starshine, the slit of the spectroscope had to be opened wider still than in the case of the zodiacal light; but gave then a closely similar spectrum; and thence we are led to the conclusion that the spectrum of the zodiacal light is the same in kind as that of either star-shine or faint sunshine. Whence the further deduction is inevitable, that the older astronomical theory of the zodiacal light being the solar illumination of infinitely small distant particles such as meteors revolving about the Sun, whether in orbits of infra or ultra planetary ellipticity, is spectroscopically maintained; while, that such solar zodiacal light has any physical connexion with the essentially terrestrial accompaniment of aurora, is just as eminently negatived by the spectroscope: for "no two spectra," as the lady most truly said in the Royal Observatory at Palermo, "can be more essentially different than those of the aurora and the zodiacal light. They are as different from one another as night from day."

Phosphorescence Spectra, Terrestrial.

Yet though our spectroscopic results so decidedly spoke for a solar origin and cosmic nature to the zodiacal light, one of the Italian mainland astronomers has published views very recently, dragging down the said phenomenon not only to the terrestrial region of the aurora, but to the still lower topographical level of meteorology and the bad weather experienced at his own Observatory in the beginning of March.

The weather with him had certainly been very bad during those days; but I had undergone some portion of it myself, having been passing along the Mediterranean from Gibraltar to Palermo at the time; and though, whenever the evening sky was free from clouds, the zodiacal light was conspicuously bright,

of said lines, combined with the increased haziness of dark lines on a darkish back-ground (a natural law well known to, and made much use of by, successful painters of night scenes): the preponderance of light belonging to the continuous spectrum, is very manifestly in the green region.

Stage 3. The green portion of the continuous spectrum is alone visible.

Stage 4. Green portion is shorter; shaded off indefinitely at either end, and culminating in what brightness it has near or about W. L. 5300.

Stage 5. Too faint now to be seen simultaneously with reference-spectrum: is seen again on widening the slit.

Stage 6. Too faint again to be seen simultaneously with reference-spectrum, even with a broad slit; but on excluding reference-spectrum the twilight spectrum may again be seen, and is found to resemble in shape, position, and all visible characters the zodiacal light spectrum as already described. The general starlight spectrum, or the spectrum of the night-shine amongst the stars above the reach of twilight or zodiacal light, is also of the same order, but fainter, and requiring a broader slit to be appreciated at all.

could not see, and the said theorist has made no attempt to prove, that it was in any way, or degree, brighter than it should have been for that latitude in that season of the year; that season, *i. e.* the beginning of March, being, let it be remembered, the long since astronomically determined date of annual maximum evening brightness of the zodiacal light for all places in the northern hemisphere.

The bad weather was accompanied no doubt over my track by a grand development of *phosphorescent* light; but that phosphorescence was in the sea, not in the sky, which, when clouded, was dark enough; and I should not think of occupying the attention of the Royal Astronomical Society with any account of it, had not that strange quality, "phosphorescence"—by name, at least,—been imported into zodiacal light discussions by so many eminent and great men; and had I not succeeded at last in doing what I believe they did not, viz., comparing such phosphorescent spectrum with that of the zodiacal light, as well directly, as through the medium of the reference-lamp.

Now, the phosphorescence here spoken of was peculiar. It was not composed of the little bright stellar sparkles, nor of the globes and discs of fainter light which are frequently seen in the wake of any vessel in many seas; but it was a broad and long trail of nearly uniform milky light, brightest close under the stern of the ship; so bright indeed there, that one could hardly but fancy that there were powerful lamps pouring forth their light from lower stern windows upon the foam-trail of a paddle-steamer. But there were no such stern windows in the "Kedar," and she was not a paddle, but a screw, steamer, keeping the blades thereof always well submerged and preserving by day a dark trail astern free from froth, foam, or bubbles, even under a noon-day sun; yet as soon as night set in, there was this phosphorescent tail, some 8 to 10 feet broad, and of indefinite length, stretching far away in luminous magnificence behind her.

At that time I had only an insufficient hand-spectroscope at command; but on finishing the zodiacal-light observations at Palermo, the same instrument, reference-lamp and all, was transferred on April 8th, to the SS. "Marathon" (belonging to Messrs. Burns and MacIver, of Liverpool), where I was to spend the next five weeks, and where the captain (John Leitch, Esq.), kindly consented to let the instrument be mounted in permanence on deck, so as to be ready for observation at any moment; a condescension to landsmen's science, in a neat and trimly kept ship, as unusual as it was in this instance both gratifying and effectual.

Night after night, however, followed, and there was for a long time very little luminosity of any kind behind this new ship; the Moon, too, was often over bright; and so it happened, that not until the 6th, 9th, and 11th days of May, in seas on either side of Gibraltar and near Lisbon, was there any opportunity of making the long-desired observation of the spectrum of light from a

similar broad sheet of milky phosphorescence to that so often seen in the "Kedar," Capt. Pritchard. The observation, however, was most satisfactorily taken on the above-mentioned days, up to the limits of power of the instrument, and gave for result,—a short continuous spectrum in the green, culminating at about W. L. 5050; or totally different from the places of both the aurora and the corona lines, much further on in refrangibility than the zodiacal-light spectrum, and having its greatest intensity towards the further, and not the nearer, end of its own light; as will be seen in the diagrams of spectrum pairs 5 and 6 in the accompanying plate of spectra symbolically represented.

Addition to a paper on Errors in Vlacq's ten-figure Logarithms, published in the last number of the Monthly Notice. By J. W. L. Glaisher, B.A., F.R.A.S., Fellow of Trinity College, Cambridge.

The main object of the present addition to my former communication is to correct an inaccuracy therein, and to add several errata with which I have since become acquainted. The inaccuracy (which is of no importance, as far as the object of the paper is concerned), is contained in the following sentence which occurs on p. 259:—"Lefort's list contains 452 errata, 301 of which are marked with an asterisk, to imply that they were given by Vega;" the words of Lefort's note are, "Toutes les fautes, relative aux nombres qui sont marqués d'un astérisque, se retrouvent dans le *Thesaurus logarithmorum completus* de Vega;" and this means, as is evident on examination of any one of the marked numbers, that the errors in question are reproduced in Vega, not that they are pointed out by him; the words, "to imply that they were given by Vega," should therefore be replaced by, "to imply that they occur also in Vega." It does not follow that the number of errors found by Vega in 452 − 301 = 151, as Vlacq's tables proceed steadily from unity to 100,000, with differences throughout, while Vega gives the logarithms of the first thousand numbers (without differences), and then the logarithms of numbers from 10,000 to 100,000, with differences; so that no errors in Vlacq's differences, &c., below 10,000, are noted; this, however, makes but slight practical difference, as this portion of Vlacq is never used.

In my paper I stated that "I had made no examination of Vega's errata-list," feeling, no doubt, that this was completely done by Lefort; but by the aid of it I have since been enabled to add to my list on p. 258, the three following errata in Vlacq's, which are not given in the *Arithmetica*, or by Lefort:—

Number.	Error.	Correction.
26517	43889	43886
30134 Diff.	1,44116	1,44119
64818	56295	56265

The first two of these are mentioned in the preface to Vega (where also reference is made to 11293, and 18723, given on p. 258), and are printed correctly in the table; the last appears in another list at the end of the introduction, and is not printed correctly in the table.

The copy of the folio edition (1794) of Vlacq, which I have always made use of, is that in the Cambridge University library; there is a preface containing a list of numbers in the logarithms of which errors were found in Vlacq; and at the end of the introduction are two pages of additional errata (in Latin and German), which the possessor of the work is requested to correct in the tables before use. The preface thus contains errors found in Vlacq before the printing of the tables, while the second list gives the result of an examination of the printed tables. All the errata in the preface are therefore errata in Vlacq, while the errata in the second list are errata in Vega, and may or may not be also errata in Vlacq: on this account, those that are also errata in Vlacq have an asterisk prefixed. The note explaining the meaning of this asterisk is not at all clear, unless the preceding explanations are premised; but it will be found that errata without the asterisk occur only in Vega, while errata with it have reference also to Vlacq, and are supplementary to the list in the preface. One or two numbers appear in both lists, and the number, 38889, should have an asterisk.

I was not a little surprised on looking at the copy of Vega (folio 1794), in the library of the Royal Observatory at Greenwich, to find that the *Corrigenda ante usum* at the end of the introduction, consisted of only one page; that most of the errata marked with an asterisk in the Cambridge copy were omitted; and that none were marked with an asterisk. The edition is also much better printed on whiter and thicker paper. The inference is, that after the publication of the table, the additional errors in Vlacq were found: the original *Corrigenda* page was therefore cancelled, and replaced by the two previously described. This affords an explanation of the fact that Lefort does not notice Vega's *Corrigenda* at all.

It ought to be mentioned that Vega does not do himself justice in his preface, as he has corrected many more of Vlacq's errors than he has called attention to; for example, errors are corrected in 38578, 48359, 40909, 42344, 41407, &c., those in the last two being important.

It is not very easy to determine in what manner Vega examined Vlacq's table, but it is very likely that he verified all the differences (and therefore all the numbers) by subtraction. Any error in the differences, unless very small, would also be rendered apparent in the course of re-arranging them as they appear in Vega. I have not yet made a detailed examination of Vega, nor have I compared side by side the two copies.

There is another error in Lefort's errata list which must be added to those on p. 258.

	Number.	Error.	Correction.
viz.	11003	29	30
which should be	11003	39	30

The origin of this inaccuracy is clear: Vlacq has 39, the 3 being a misprint for 2; Vega corrected this, and printed 29; the remaining unit error of course is an error of calculation.

It would be very useful to examine all the errors in Vlacq which could affect a table of seven-figure logarithms, and give these in a list by themselves: such a list would occupy little space, and would be very valuable, as by means of it, anyone could easily render any seven-figure table free from the hereditary errors that have descended from Vlacq. In a paper entitled, "Notice respecting some errors common to many tables of logarithms" (*Memoirs of the Royal Astronomical Society*, vol. III. 1829), Babbage has noticed six errors that occur in Vlacq, and have been reproduced in nearly all subsequent tables. Out of 22 tables which he has included, the errors occur in all that are intermediate to Vlacq and Vega, and in several that are more recent, the only ones free from them being the Vegas, the later editions of Callet and Hutton, and Babbage. Even tables published since Babbage's contain some of these errors: thus five out of the six appear in Shortrede, and one in Sang's tables (1871). Another of Vlacq's errors also appears both in Babbage and Sang (see *Athenæum*, June 8 and 15, 1872), so that even by editors of logarithmic tables all the Vlacq's errors that have been published are not known.

This suggested list of errata in seven-figure logarithms, which, by the aid of Lefort, can now be rendered probably really complete, I intend to form at once.

The following errata in former paper may be noted here, viz. p. 258, line 4, "Diff" should be inserted after 3192; p. 259 (note), Hatton should be Hutton, and p. 262; "Leipsiæ" should be "Lipsiæ."

Eclipse Photography. By A. Brothers, Esq.

It may, perhaps, be thought remarkable, that the photographs taken in India during the Eclipse in December last, do not show the outer corona, and its absence from the negative would almost lead to the supposition that the outer rays had no existence during this eclipse. But as we have the evidence of Captain Tupmann, that he traced those outer rays to about 40′ from the Moon's limb, the inquiry naturally arises, why do not the photographs show this part of the phenomenon?

I am strongly of opinion that any such effect of light visible to the naked eye, if of sufficient duration, can be photographed; and, as the outer corona is persistent during the whole time of

totality, and clearly visible in the No. 5 Syracuse negative, it is, I think, desirable to consider the reasons why this single photograph shows an effect which is not seen in any of the photographs taken at Bekul, or at Dodabetta.

The No. 5 negative was exposed 8 seconds, and was developed in the ordinary way with iron, but not strengthened, care being taken that the development should not be pushed too far so as to obscure detail. As the denser part alone was visible in the dark room, it was only on the negative being viewed in the full daylight that the faint part of the corona could be seen, and then chiefly by reflected light.

That the effect on the plate is really a picture of the corona is proved in various ways. The light shades off gradually from the bright parts of the corona, and there is no photographic effect where the rifts occur. The faint corona was visible to the unaided eye, and was identical with the appearance photographed. Now as the negative is altogether untouched, why is it that it shows an effect which none of the Indian negatives exhibited? The reason appears to be that the difference is probably due to manipulation. The No. 5 negative shows what was actually visible, but it happened that the exposure and developments were just right for bringing out such detail.

A careful examination of Mr. Davis's negatives will show the cause of the difference between them and the Syracuse picture.

The Bekul negative No. 1 was exposed 15 seconds, and shows a corona extending about 15′ from the Moon's limb.* No. 2 was exposed 10 seconds, but shows a corona of only about 8′ extent. No. 4, however, was also exposed 10 seconds, but shows the coronal rays extending in some directions about 20′, with indications that they reached a still greater distance from the Moon. As the conditions were, I believe, the same during the whole time of the eclipse, there would be no variation in the light to account for these differences, and we must fall back on the manipulation and time of exposure for our explanation.

As I have not seen Colonel Tennant's photographs, I cannot refer to them in detail, but it has been stated that they present very much the same appearance as those taken by Mr. Davis, and my remarks therefore refer to the Bekul negatives only.

Those of the Fellows of this Society who are familiar with the practice of photography, will at once understand what is meant by manipulation; but to make my intention quite clear, I have prepared three photographs to illustrate the matter. They are copies of my drawing from the Syracuse negative. The exposure for each picture was the same; 5 seconds, and in the same light. The print marked A presents only the bright part of the corona; B shows the proper effect of the drawing; and C shows the effect

* I am speaking with reference to the transparent copies kindly sent me by Lord Lindsay. The original negatives will probably show different measures, and other transparent copies will differ; those I have, are very perfect and beautiful; but the argument is the same in any case.

of pushing the development too far.* The examination of these photographs leaves no doubt that such a faint object as the outer corona would only be depicted on the plate when the development and time of exposure had been exactly suited for it — under-development in the one case would prevent its appearance on the plate; and in the case of over-development, it would be quite possible to obliterate all trace of it.

Seeing then that of the six or seven good pictures taken during the late eclipse, not one shows the full extent of the outer corona, it must be concluded that the operators have not succeeded in so accommodating the exposures and development as to obtain the result which the Syracuse picture fortunately, but as I am inclined to believe accidentally, shows.

The failure, however, in obtaining the full picture of the corona, may possibly be explained in another way. It was early morning when the Bekul photographs were taken; and at Dodabetta the eclipse was observed through a mist. This mist was not sufficient to obscure the details of the inner corona, but photographically the interference may have been fatal for the fainter rays. Those who have attempted to photograph the Moon know very well the effect of the slightest mist in the atmosphere; and it must be borne in mind that the outer coronal rays are fainter, very much fainter, than the light of the full Moon.

At the last Meeting of this Society, Colonel Tennant is reported to have said, that "of course" the photographs do not show all the eye could see. From what I have said, and from the fact that the Syracuse picture does show what the eye saw, I cannot entertain Colonel Tennant's opinion. What the eye can see can be depicted photographically (the atmospheric conditions being favourable), but I am not at all sure that we have hitherto adopted the proper means to secure the best pictures of an eclipse. It is probable that the Syracuse photograph is one of the fortunate successes which occur in the experience of every photographer. Following the same means, it is not certain that the same thing can be done again; but there is strong reason for the opinion, that by using the old, but now almost forgotten *positive* process, the kind of picture required could be obtained with certainty. A positive on glass gives the picture by reflected light; but if the picture be viewed by transmitted light, we have the effect of a negative. We by this means secure two objects—the brighter corona, which requires so much less time to impress its details, would be seen by transmitted light, and the full picture by reflected light.†

The opinion has been expressed that better work could have

* These photographs were exhibited at the meeting, and will be again shown at the meeting of November 8.

† With the box of chemicals, &c. which I prepared for Colonel Tennant, I sent everything requisite for working the positive process, thinking it probable that some of the pictures might be taken in the way I suggested; but as only negatives have been spoken of, I conclude the suggestion was not acted on.

been done with a reflecting telescope during the last eclipse, than with the rapid rectilinear lens used by Colonel Tennant and Mr. Davis. When we bear in mind what has actually been done by the various classes of instruments, it is difficult to find a reason for the opinion referred to. It cannot be said that the refracting telescope has altogether failed, as I have before me as I write, a very beautiful photograph of the corona, printed direct from the original negative taken at Shelbyville by Mr. Whipple, in August 1869. We have also the picture taken in Spain in 1870. The Shelbyville photograph is $\frac{7}{8}$ of an inch in diameter; it was exposed 40 seconds, and shows a corona extending about 6′. The Cadiz picture is less perfect, but it was exposed $1\frac{1}{2}$ minutes. In these two photographs we have the best work hitherto done with the ordinary telescope. (I do not refer to the 1860 photographs, because the prominences were the chief objects to be depicted, and the corona shown is very limited in extent.)

The silvered-glass reflector was used first in India in 1868, for the purpose chiefly of photographing the red prominences, and the negatives were obtained with exposures ranging from 1 to 10 seconds. Judging from the plates engraved in the *Memoirs* of this Society, the longest exposure was not sufficient to show the corona. A similar reflector was used by Lord Lindsay in Spain, but from some reason not hitherto explained (possibly owing to atmospheric causes) the corona is not seen in a satisfactory manner. It is probable, however, that owing to the focus of the telescope being short, as compared with the aperture, there is more light around the dark Moon than in any other eclipse photograph taken with a reflector.

The larger image obtainable with the instrument of long focus is its only advantage. As, however, a longer exposure is requisite, we lose in one way all the advantage gained in the other. Instruments of long focus are objectionable, on account of the large mounting required, and for other reasons.

On the whole, it appears to me that the evidence is in favour of an achromatic object-glass. I do not maintain that the rapid rectilinear lens is the best that can be had, but it is possible that a lens of similar construction of large aperture, and giving a picture of the Sun of about half an inch in diameter, would work more satisfactorily than a reflector of equal proportions. Such instruments, as I have indicated, could be tried side by side on the Moon. The best effect with the shortest possible exposure are the points to be tested. The most perfect driving clock in the world will not avail against atmospheric disturbance.

When the *extent* of the corona is referred to, it would be advisable, in all cases, if the observer would state whether a naked eye or telescopic view is referred to, as it appears to be pretty certain that the telescope fails to show more than the brighter parts.

It should be distinctly understood that in what I have said I disclaim all intention of undervaluing the work of others, as I

think what has been done is so near perfection that it is difficult to conceive anything superior to be done, when the particular character of the corona is taken into consideration.

June 12, 1872.

On an Altazimuth Mounting for Telescopes, especially adapted for the use of Observers who have no permanent Observatory. By John Brett.

The engraving intended to illustrate this paper represents a reflecting telescope of the Newtonian form, of 9½-inches aperture and 6 feet 2 inches focus, the mounting of which exhibits certain peculiarities intended to improve the performance of such instruments, as well as certain contrivances calculated to render them

more portable than usual, and more readily available for general use. The principle upon which this mounting depends is applicable to any sort of telescope; but inasmuch as steadiness and smoothness of movement are of comparatively easy attainment in refractors, I propose only to lay before the Society my adaptation of it to reflectors.

The telescope is supported at both ends; the pivot on which

it revolves is placed at the lower end immediately under the speculum; and the power by which it is moved and governed is applied as far as possible therefrom, that is to say, at the eye-piece end of the tube. This disposition is intended to prevent any tendency to oscillation, and to check any vibration that may be set up by the wind or by any accidental cause.

The whole apparatus, telescope and stand, may be briefly described as a tripod with a moveable apex; and the essential peculiarity (and, as I suppose, novelty) of the mounting consists in the means by which the apex is made to move in any required direction.

One leg of the tripod consists of the tube or body of the telescope, the speculum end of which rests on the ground, whilst the eye-piece end, being situated at the apex, is supported by the other two legs.

The telescope having a given length which is invariable, it is obvious that it may be made to stand at any angle to the horizon by the extension or shortening of the other two legs. It is further evident that if these two contractile legs be shortened or lengthened at the same rate, the telescope or third leg will be moved merely in altitude; that if one of them only be extended or reduced the telescope will describe an arc of a circle; whereas, if one leg be lengthened and the other shortened at the same rate, a lateral movement will be communicated to the apex, that is to say, the telescope will move in azimuth only.

It will be seen that the figure of the whole apparatus is that of the simplest of all solids, and since its movements depend entirely on thrust—torsion being wholly excluded, and friction reduced to a very small amount—it is hardly possible to manufacture it so weakly as to produce unsteadiness.

The mounting provides for quick and slow motion both in altitude and in azimuth, besides which it affords a slow motion in a circular direction, which latter may be made equatorial by placing the speculum end or pivot of the telescope on a bench or other support so many degrees higher than the casters of the other two legs as shall equal the latitude of the place of observation. The quick motion in altitude is given by an extension of the two supporting legs, the upper halves of which (consisting of two similar brass tubes about an inch in diameter and about 28 inches long in the particular instrument referred to) are made to draw out of the lower halves, which consist of mahogany cylinders or boxes carrying clamping-screws to fix the brass tubes at the required height. The quick motion in azimuth is given by simply moving the whole tripod on its casters. The slow motion in every direction is communicated by two double-threaded steel screws (18 inches long), which work in the brass tubes and are moved by the observer. The heads of these screws terminate in ball-and-socket joints permanently fixed to the tube of the telescope. Both these screws may be turned at the same time by the observer using both his hands, and the result will be that when both

are moved in one direction the telescope rises in altitude, or in the other direction it descends; but if they are turned in contrary directions the instrument moves in azimuth. The rotation of one screw alone produces a circular motion which, affording as it does a certain approximation to the apparent motions of the stars, allows the observer to keep the object in the field with greater facility than the ordinary altazimuth mounting does, whilst the equality of force required to rotate either of the screws and the smoothness of their action allow the object to be followed without any care or attention, and far more steadily than by the usual Hook's joint.

In this instrument the legs are connected together by slight bars of wood which can be readily folded along with the legs; and when not in use the whole stand, together with the finder, nose-piece, eye-pieces, and all other appendages, retire into the seclusion of the telescope-tube, the door of which shuts them off from the meddling of the curious by closing with a spring-lock. The tube in this instance has been made rectangular, as the cylindrical form was supposed to lend itself favourably to vibrations. The upper side is covered in with panelling to prevent the deposition of dew, but the other sides are filled in with cane-work to prevent air-currents. The door at the upper end is made to open so as to bring the nose-piece exactly opposite to the plane mirror. The door at the lower end allows the cover to be taken off the speculum. The little door underneath carries the eye-pieces, and keeps them close to the observer's hand and away from his pockets. The hollow legs are furnished with spiral springs, which form a counterpoise of increasing power as the weight of the telescope is thrown forward upon them. Here ends the description of the instrument.

It being generally recognised that unsteadiness is alone sufficient to render the best of telescopes utterly useless; that reflectors are peculiarly liable to suffer from this defect; that in order to avoid it great weight and massiveness have almost always been resorted to in their mounting, and that this cumbrousness tends very much to restrict the use of large reflectors to permanent observatories, thereby crippling to some extent the observing power of this Society: supposing that these things are generally admitted, I have thought the characteristics of portability and steadiness to which my principle lends itself with a certain aptness give to this apparatus sufficient claims to attention to warrant my producing it for the inspection of the Fellows; and, if they do not think favourably of the design, they will, I feel sure, appreciate the way in which Mr. Browning has carried it out.

6 Pump Court, Temple,
May, 1872.

On certain Phenomena surrounding the Sun's Limb as seen in the Telescope. By John Brett, Esq. (Abstract.)

The appearance to which attention is called may be briefly described as follows:—

When the Sun's disk is examined with a telescope it is seen to be projected, not against an even background of sky-colour of uniform darkness, but upon a patch of weakly luminous halo diluted gradually outwards to an extent of about half a diameter of the Sun beyond his limb, where it dies away and becomes imperceptible. The colour of this halo is of a delicate steel grey with a slight hint of green in it. Its inner region next the limb possesses a certain brightness for about 2′ or rather less, exhibiting some traces of a defined limit at that distance, beyond which the diminution of its light proceeds rapidly. The aperture of the instrument does not seem to affect its visibility much, but the dark glass must not be darker than is necessary for safety. The inner or brighter belt has been seen best with a magnifying power of from 50 to 100 on Mr. Brett's 9½-inch telescope, but, speaking generally of the whole phenomenon, the lower the magnifying power and the larger the field the more perfectly it can be seen. In a small finder, with field of 4° and magnifying power of 5, its appearance is remarkably vivid and strikingly like Mr. Brothers' photograph of the Corona of 1870, and on some occasions appearances exactly like the so-called rifts have been seen.*

By fixing a small strip of paper across the field-lens of a Kellner eye-piece so as to make a bar in the focus across the image, and afford a standard of contrast, the graduation of the light from the limb outwards was made to appear more distinctly, and since that date Mr. Brett has never examined the Sun's limb in good clear sky without detecting a similar appearance.

Mr. Brett has tried all the means he can think of to get rid of it, but in vain. He has shown it to several persons with good eyes (in various telescopes, with all sorts of eye-pieces and different dark glasses, first-reflection prisms, and films of silver), and most of them describe the phenomenon as perfectly obvious.

Mr. Brett invites attention to his paper in the *Notices* of this Society for March, 1871, in which is recorded some account of the Corona as seen during the progress of the eclipse of December, 1870, especially during its earlier stages. On that occasion the Moon's limb outside the Sun was seen by Mr. Brett and Mr. Burton projected dark on a brighter background of sky many minutes before the Sun's disk was covered.

6 Pump Court, Temple,
June, 1872.

* In a letter to Dr. Huggins, Mr. Brett mentions that the rifts thus seen are not permanent.

[The phenomena described by Mr. Brett, though not altogether new, afford an interesting subject of inquiry. The comparison of such phenomena as presented when different instruments are employed, when the Sun's image is in different parts of the field, and so on, may possibly throw light on questions relating to the performance of telescopes. It may be, on the contrary, that the phenomena are caused by the presence of matter in the upper regions of the atmosphere; that in fact it is a true atmospheric corona, and related to finely-divided matter very high in the atmosphere, in the same way that the common atmospheric corona is related to the water vesicles of *cumulus* or *cumulo-stratus* clouds. It seems by no means unlikely, indeed, that the phenomenon described by Mr. Brett is the "corona" caused by the particles which form the *cirrus* or *cirro-stratus* cloud. It may be interesting to compare Kaemtz's account of the corona of the cumulus or cumulo-stratus cloud. "If the corona is complete," he says, "several concentric circles are observed. Near the Sun they are of a deep blue, the second circle is white, and the third red, which terminates the first series; in the second we see, still going in the direction from the centre to the circumference, purple, blue, green, pale yellow, and red; the series is rarely thus complete. More frequently we observe near the Sun blue mingled with red, then a white circle clearly limited within, but confounded without with the others. If a second red circle exists outside this, then green is observed in the interval by which they are separated. The distance of this circle from the centre of the Sun varies according to the state of the clouds and atmosphere; I have found it from 1° to 4°." If we remember that such phenomena may be produced by reflected as well as by transmitted light, we see reason for believing that cirrus clouds, according to the usual belief as to their constitution, would produce a real though small corona as well as the haloes which it is their more special part to produce. In thus speaking of cirrus clouds, we have in view as well the dispersed matter which is almost constantly present in the upper air, though not recognisable in distinct cloud forms, as cirrus clouds more properly so termed.

It may be, however, that the phenomena are altogether optical; or, again, they may be merely subjective.

It is hardly necessary to show that the phenomena are not astronomical,—in other words, that the astronomical solar corona has not been seen. Dr. De La Rue has shown how hopeless it must be to attempt to see the prominences (whose light is certainly much brighter than that of the corona, even a few minutes only from the limb):* see vol. xxix. of the *Notices*, p. 81. The Astronomer Royal has tried the following experiment:—A smooth white surface, with a circular aperture, was so placed that

* In 1860, Goldschmidt could perceive a prominence which he had been watching, $3^m\ 40^s$ after the reappearance of the Sun, and believed he could have seen it longer had he not been compelled to withdraw from the observation.

the aperture exactly corresponded to the place where the Sun's image would fall, and the light thus passing through the aperture was quenched in a black bag. When this was done not a trace of the prominences or chromatosphere could be recognised round the aperture. If under these exceptionally favourable circumstances even the prominences could not be seen, it is certain that the corona cannot be seen when the Sun is not eclipsed (unless with some spectroscopic appliances not yet tried). It is sufficient to remark, however, as Lord Lindsay pointed out at the last Meeting, that since the corona cannot be seen through a darkened glass (such as is used in observing the Sun) during total eclipse, though then projected on a dark sky, it cannot possibly be seen through such a glass when the sky is illuminated by the full blaze of sunlight.

While on this subject we may mention a method by which possibly the whole circle of the prominences and sierra might be seen, either with red, yellow, green-blue, or indigo light, when the Sun is not eclipsed. It consists simply in repeating the Astronomer Royal's experiment, but viewing the ring or card through a train of prisms (after the manner proposed by Prof. Young early in 1871 for observing the corona, and applied by Mr. Lockyer during the last eclipse.) Theoretically it appears certain that the complete ring of prominences and sierra should thus be rendered visible at once. Or Sir George Airy's experiment could be repeated with the aid of the spectroscopic arrangement successfully employed by Respighi at Poodoocottah last December. Either method might probably be applied successfully to obtain photographs of the prominences and sierra, the indigo image being employed. It seems barely possible that a faint image of the corona might be detected by this method at the part of the aperture corresponding to Kirchhoff's line 1474. If the method failed under ordinary circumstances, it might be successful at a station very high above the sea-level. The present writer has been confirmed by the high opinion of the Astronomer Royal (to whom this extension of the black-bag method had also occurred), in his belief that valuable results might be obtained from the application of this method.

As regards the visibility of the lunar limb outside the Sun many minutes before or after totality, or in partial eclipses, it is only necessary to mention that theoretically this is to be expected. In an article in *Fraser's Magazine* for December, 1870, the possibility of thus detecting the Moon's limb outside the Sun's during the partial eclipse visible in England during that month was pointed out (before the event) by the present writer, who also can confirm Capt. Noble's statement that the lunar limb can actually be seen on such occasions. It is to be remembered that so soon as a solar eclipse commences the illumination of the atmosphere begins to be reduced, and that at an advanced stage of the partial phase this reduction becomes such that, under favourable atmospheric conditions, the Moon's limb

must necessarily become visible by its relief against the light of the corona (some time before the corona itself can be seen.)—R. A. P.]

Improvements in Tripod Stands. By R. T. Lecky, Esq.

My object in submitting to the Society so very simple a matter as a tripod-stand is, firstly, to place on record the invention of the well-known double, or, as it is commonly called, the French Tripod, so universally used under various forms for photographic cameras, portable telescope-stands, &c.; and, secondly, to show some further changes in its construction which may be usefully employed in various astronomical and geodetic appliances.

The invention of the double-stand is that of a gentleman to whom the scientific world is indebted for having laid the foundation of the vast improvements of later years in the construction of the object glasses of microscopes, the late Joseph Jackson Lister, Esq., F.R.S., of Upton, Essex, and the stand which I have the pleasure of showing here to-night (kindly lent me by his son, Mr. Arthur Lister) is the first of its sort made in this country about forty years since by the late Mr. Bate, of the Poultry.

Mr. Lister was fond of sketching, and although well able to use his pencil unaided by any optical appliance, used the camera lucida for greater accuracy and speed, and as the ordinary walking-stick-stand was both cumbrous and inconveniently low, he contrived this to carry a small drawing-board, and to fold up and be used as a staff in his walking excursions. The upper portion of each leg is divided, and the short part is hinged to the side of the leg, and the two parts are then connected with a jointed link, which on shutting-up the stand fits into a recess in the side of each part, and the legs are then connected together with ferules, and a movable ring in the usual manner so as to form a light and useful staff. When the legs are opened out, the drawing-board is pinned on to the top and makes a firm table. Mr. Bate was so pleased with the invention that he requested Mr. Lister to allow it to be patented, but this Mr. Lister declined to do, and gave it to the public.

A few years since, on Mr. Lister showing me this stand, it occurred to me that if an ordinary level or theodolite tripod could be adapted to the same use by having a drawing-board braced on to its top; it might be made useful for a greater variety of purposes, and be readily made of materials common to every collection of instruments. I therefore tried a few experiments with the stand I have the pleasure to show you, and the result has been that by placing the board on a loose centre on the top of the tripod, and then applying two cross braces between the

side of the board and the two front legs, and two other braces as an isosceles triangle to the third, or back leg, and then connecting the legs together, the connexion between the front legs being rigid, I made a very firm stand or table, and with the unexpected result, that whereas, to use engineering terms, the first six braces, viz. the four between the sides of the board and the legs, and the two connecting the two front legs to the third or back leg, act as ties, the seventh or rigid connexion between the two front legs becomes a strut, and, as such, its action is to tighten all the other ties or braces, thus giving the table the requisite degree of stability.

The braces I have in this case made of spring steel for lightness, but they may be of chain or wire, and if with the strut were made adjustable, by screws or otherwise, the table could be adjusted as to level, or to any required degree of obliquity.

As to the different uses to which a stand of this kind is applicable, I may mention the old-fashioned but useful surveying instrument, the Plane Table, which, with this mode of bracing, may be made of much larger size than is usual, and be constructed with greater accuracy. It would also form a firm and convenient stand for various uses in the observatory as well as in the field; light, portable, and easily taken asunder and stowed in a small space, at the same time not interfering with the other uses of the tripod. Doubtless other uses will suggest themselves in practice.

Memoir on the Theories of the four Superior Planets, Jupiter, Saturn, Uranus, and Neptune. By M. Le Verrier.

An abstract of a memoir by M. Le Verrier on the theories of the four exterior planets is inserted in No. xxi., for May 20, 1872, of the *Comptes Rendus*. The subject is of great importance, and one deeply interesting to many Fellows of this Society. The following is a translation of the principal portion of the abstract.

"On several occasions I have had the honour of laying before the Academy a series of investigations relating to the system of the four planets nearest to the Sun; viz., *Mercury*, *Venus*, the *Earth*, and *Mars*. Although at an earlier period, I had already been occupied in the study of the larger planets, I have felt the necessity, before continuing the investigation, of establishing on a solid foundation the theory of the *Earth's* motion, which serves as the basis of all the others. This study led me to the consideration of the three planets situated nearest the *Earth*, and which constitute the inferior portion of the planetary system.

"These researches have shown that the motions of the *Earth* and *Venus* are represented by the theory with all the accuracy which the observations will admit of. *Mercury* and *Mars*, however, showed some irregularities.

"The difficulties which the last two planets presented to us have in a great measure disappeared after a careful examination of the subject. In order to establish an agreement between the theoretical and observed results, we have come to the conclusion that it would suffice if we were to increase the secular motions of the perihelia of *Mercury* and *Mars*, which would imply the existence in the neighbourhood, both of *Mercury* and *Mars*, of sensible quantities of matter of which we hitherto had no knowledge.

"With regard to *Mars*, these deductions have been confirmed. It has been proved that the matter, of which no account had hitherto been taken, ought to have been added to the *Earth* itself; the estimate of the mass of our planet being $\frac{1}{8}$th too small.

"With regard to *Mercury*, this confirmation is not yet complete. Several astronomers have placed on record the passage over the *Sun's* disk of certain small bodies, which could be nothing else but very small planets, but it has not been possible to determine the orbit of any one of them. Whether we have to deal with the action of a certain number of small masses, or with that of matter distributed in the neighbourhood of the *Sun*, the theory of *Mercury* has been determined with considerable care, and the transits of the planet over the *Sun's* disk furnish us with observations too precise to admit of any doubt of the accuracy of our results; especially as they have been obtained in the same manner as for *Mars*, and for this latter planet the confirmation which the theory has received leaves nothing more to be desired.

"Similar researches into the system of the four large planets farthest from the *Sun* would be interesting, as they would furnish us with data concerning any matter, still unknown to us, that may be situated in those regions. It is not impossible that, by these means, we might succeed in proving the existence of some planet beyond *Neptune*, and in limiting the space in which a search for such an object may be confined. At any rate we should have collected the materials necessary for the more speedy accomplishment of future discoveries.

"The first part of the work to be undertaken consists in the determination of the disturbances which each of the four planets experiences from the action of the others. It is this first part that I present to the Academy to-day.

"The respective theories of *Jupiter* and *Saturn* are not free from difficulty on account of the magnitude of the masses of these planets, and because the ratio existing between the mean motions of *Jupiter* and *Saturn* is very nearly represented by 5 to 2. Under these circumstances, some of the terms rise to a very considerable value, which requires the approximations to be carried farther. Besides which, the series of multiples of the angles, by following which the perturbing functions and perturbations are developed, have to be carried very far to avoid neglecting any term of the longitude exceeding one hundredth of a second, as we resolved to do. Again, the terms depending on the second order

in relation to the perturbing masses, become very sensible, and their determination is a laborious operation.

"Two methods may be adopted for the determination of the inequalities of the first and second order in terms of the masses.

"The method of *interpolation* has this advantage; the actual work of the greater part of the investigation may be given to a computer; the numerous checks which present themselves preventing the possibility of any serious error escaping notice. On the other hand, this method has this inconvenience, that the computation being possible only for numerical data, everything has to be recommenced as soon as these data have undergone any considerable change depending upon the secular variations of the elements of the orbits.

"The development of the perturbing functions, as well as that of the inequalities of the elements under an algebraic form, in which the terms which vary with the time are left indeterminate, leads, on the contrary, to expressions which will serve for all succeeding ages. This is the course which it appeared to me necessary to follow in order to render a really important service to astronomy.

"Consequently, in the expressions I have adopted, the excentricities, the inclinations, and the longitudes of the perihelia and nodes, are, like the mean longitude, left in the form of variable quantities. The mean portions of the major axes are alone treated as given numbers, as is quite allowable, since these undergo no secular variation.

"The first chapter is devoted to the development of the transcendentals on which the determination of the coefficients depends.

"In the second chapter we give the algebraic development of all the terms of the perturbing functions which occur in the theory of *Jupiter* and *Saturn*.

"The terms of the great inequality depending on five times the mean motion of *Saturn*, less twice that of *Jupiter*, are developed with care to the seventh order.

"The third chapter contains the numerical expressions peculiar to *Jupiter* and *Saturn*.

"In the fourth chapter the perturbations of *Jupiter* are determined to the first order in relation to the mass of *Saturn*. For example, for the part of the great inequality depending on the angle

$$5\,l' - 2\,\lambda - \varpi' - 2\,\omega$$

the following expression is found:—

$$-(1+\mu')\begin{pmatrix}791''\cdot32\,\beta^2\beta' - 2''\cdot00\,\beta^4\beta' - 15''\cdot06\,\beta^2\beta'^3 - 1''\cdot25\,\beta^2\beta'\varkappa^2 \\ +\,0''\cdot07\,\beta^4\beta'^3 + 0''\cdot12\,\beta^2\beta'^5\end{pmatrix}\text{sin.}\begin{pmatrix}5\,l' - 2\,\lambda \\ -\varpi' - 2\,\omega\end{pmatrix}$$

in which $1+\mu'$ represents the real mass of *Saturn*; β and β' are the ratios of the excentricities of *Jupiter* and *Saturn* at any

given epoch, to their values at the fundamental epoch fixed at mean noon on January 1, 1850; $\varkappa$ is the ratio of the mutual inclination of the orbits of *Jupiter* and *Saturn* to the value of this inclination at the fundamental epoch; ϖ' and ϖ represent the longitudes of the perihelia of *Saturn* and *Jupiter*.

"The fifth chapter contains the inequalities of the elements of *Saturn* in the same form.

"The inequalities of the second order in terms of the masses are sufficiently sensible to make a careful determination of them important; the fifth chapter is devoted to this object. It is necessary to put them in the same form as for those of the first order, so that by combining the whole of them we may have the complete expressions for the periodical variations of the elements in the form in which they may be easily referred to any given epoch.

"On account of the magnitude of the perturbations of the first order, and the smallness of the divisors $2n' - n$, $3n' - n$, $5n' - 2n$, and $10n' - 4n$, a large number of terms of the second order are sensible; they depend on the combination of different groups, and in each of these groups there are a considerable number of terms of which notice must be taken, if we wish to obtain an accurate result.

"For example, the term depending on the angle $5l' - 2\lambda - 2\varpi - \varpi'$, corresponding to that which we have already given to the first order, depends on not less than 18 combinations of groups, taken two and two, in each of which about 10 combinations of terms occur. The total gives an angle of 17 seconds for the co-efficient.

"The determination of the complete expression of these terms has been brought into a form, which, though leaving it complicated, is one in which we may proceed securely, leaving the variable elements indeterminate as for the first order.

"But how can we feel certain that in the course of such numerous operations no error has been committed?

"In the algebraic form, by attributing to the two planets determinate mean longitudes; for example, by placing the planets at their perihelia or their aphelia, we can obtain simple expressions of the corresponding value of the perturbing function, and thus deduce relations between the co-efficients of the general expression of this function, which form a valuable means of verification. These verifications, carefully made *à posteriori*, have been all found exact. The only exception has been for a very small term of the perturbing function of *Saturn*, arising from the reaction of *Jupiter* upon the *Sun*; a term of a very high order, quite insensible, which had changed its sign.

"In the numerical form, although all the co-efficients of the developments have been three times revised, we wished to determine their sums anew by the method of interpolation, of which we have already spoken; and we have even continued the work far enough to form an independent determination of the question

we are discussing. This second investigation is in complete accordance with the first.

"We may then hope that astronomers will accept with confidence the work which we now place before them. It is our intention to print elsewhere all the details necessary to enable any person to verify the parts which he may be interested in examining more closely. In this manner, should a doubt arise on any point, it will easily be seen in what direction the work of verification should be directed.

"The two succeeding sections comprise the theories of the perturbations of *Jupiter* by *Uranus* and *Neptune*.

"The fourth contains the perturbations of *Saturn* by *Uranus* and *Neptune*.

"Then follow three sections, containing the perturbations of *Uranus* by *Jupiter*, *Saturn*, and *Neptune*. The investigation of the perturbations of *Uranus* by *Saturn* was made many years ago, but I then followed the method of interpolation. In the present work, in which the results agree with the former one, I have adopted the algebraic form, which can be applied to any epoch.

"The last sections contain the perturbations of *Neptune*, produced by *Jupiter*, *Saturn*, and *Uranus*.

"To bring this work to a satisfactory conclusion, it will be necessary,—

"1. To calculate the formulæ, and to reduce them into the form of provisional tables.

"2. To combine all the accurate observations of the four planets, and to re-discuss them in order to refer their positions to the same system of co-ordinates.

"3. By means of the provisional tables to calculate the apparent places of the planets for the epochs of the observations.

"4. To compare the observed with the tabular places; to deduce the corrections of the elliptic elements of the four planets, and to determine if the agreement is then perfect.

"5. Should the agreement not be satisfactory, to investigate the cause of the discordance."

Observations of the Planet Venus, with a 6-inch Silvered Glass Reflector. By R. Langdon, Esq.

(*Communicated by J. Norman Lockyer, F.R.S.*)

On May 1st, 1871, I had a good view of the planet *Venus*; but I could not at first see her to my satisfaction as her light was so bright. She had more the appearance of a miniature sun than a star; but I put a diaphragm of blackened card in the eye-piece, and made a small hole through its centre with a piece of hot wire. I found this arrangement keep out to a great extent the glaring rays. I also sometimes used a slip of slightly-tinted glass in

front of the eye lens; this enabled me to bring the planet entirely under subjection. Her shape was that of the Moon when a little more than half full. I distinctly saw a dull, cloudy-looking mark along her bright limb, curving round parallel to it, extending nearly across the disc, each end terminating in a point; joining this at the eastern extremity, was another and darker mark of a club shape, its small end joining the point of the mark previously described. (See Sketch No. 1.) I watched these marks for half-an-hour; I saw some marks again the next evening, but before I could examine them the planet was hid behind some clouds.

On May 6th, at 7.45 P.M., there was a cloud-like mark extending straight across the disc, and a club-shaped mark nearly in the centre, with its small end nearly touching the straight cloud. On the western limb another dark mark had made its appearance; it was not quite so large as the other, and it was not club-shaped; but its sides were parallel to each other till they approached the

straight cloud, when they appeared to divide, each side curving round away from the other. (See Sketch No. 2.)

I took much interest in watching these spots, as I had read that "it was very doubtful whether any marks had ever been seen on this planet." I called several men to look at them, and they were able to describe them, although they had no previous knowledge or idea of what they were likely to see. One man was very confident it was the Moon he was looking at, but when I pointed out to him the Moon was not in the neighbourhood, he said he thought it was the Moon, because he could plainly see the dark patches on its surface.

On May 13th, at 7.30 P.M., there was a dark mark of a pear shape, extending from near the western edge to two-thirds the distance across the bright disc. This mark was not so dark as

those seen on the 1st and 6th, but it was much larger. (See Sketch No. 3.)

On July 28th, at 8 P.M., there were visible five dusky marks along the planet's terminator, and one nearly in the centre of the crescent; but they were not so well defined as those before described; but what seemed to me more remarkable, was that the southern horn was rounded off considerably, whilst the northern horn was quite sharp, and ran out to a very fine thread-like point. (See Sketch No. 4.)

On October 13th, at 5.45 A.M., I saw *Venus* as a beautiful little crescent. She was well defined, and both horns were as sharp as the finest-pointed needles. I think I detected a dusky cloud-like mark about half way from the centre to the northern horn; but I am not quite sure about this, as I had to leave the telescope before I could complete my sketch. (See Sketch No. 5.)

On October 25th, at 8.10 A.M. On this occasion I was gratified with a sight which I had waited for and longed to see for many years; that was to have a good view of *Venus* by daylight. I now had the longed-for opportunity, and it turned out as I expected it would—that the superior light of the Sun would overcome that of the planet to such an extent as would enable me to see her better than I had ever seen her before. I could now plainly perceive the jagged nature of the terminator, the unevenness of which could not be mistaken; but what was very remarkable, the northern horn was bent in towards the centre of the planet; it appeared as if a notch had been cut in the inside, and a slice cut off from the outside. (See Sketch No. 6.)

I have no idea what was the cause of this appearance; I have never seen it so before, neither do I recollect ever having read of such a phenomenon. I did not perceive any markings on this occasion, but there was a kind of haziness along the whole length of the terminator; but I considered this at the time to have belonged to the terminator rather than to any markings on the disc. The terminator on this occasion was inky black.

November 9th, I saw *Venus* every half-hour during the day up to 1 o'clock. I made a sketch at 12.20 P.M. I could now distinctly see the jagged terminator, the nature of which was so much like that of the Moon as it was possible to conceive, except that if we compare the Moon's terminator to a piece of network, that of *Venus* would be represented by a piece of fine lace. I could also see some thin, cloudy marks on her disc. The southern horn was very sharp; the northern one was a trifle rounded.

I saw *Venus* on February 5th, 1870 (a few days before her inferior conjunction with the Sun), the bright part was an exceedingly beautiful fine crescent; but myself and several other people could see the whole body of the planet in the same manner as we see the dark limb of the Moon when *Earth-shine* is falling upon it; but I did not make any sketch at the time.

I have observed *Venus* a great many times besides those mentioned above, having made it my special work to do so, and

have on several occasions strongly suspected markings to have been visible; but I have not mentioned them, and have only described those times upon which I have had no doubt of what I had seen.

Silverton Station, near Cullompton, Devon.

New Tables of Uranus. By Simon Newcomb.

(*Extract from a Letter addressed to the Astronomer Royal.*)

"When I last enjoyed the pleasure of writing to you, I made known that I had long been engaged in perfecting the theory and tables of *Uranus*. After devoting a large amount of labour to this subject during the last twelve years, the work is now, I hope, approaching its close. I have fixed upon the opposition of 1871–2, as the last one on the observations of which the tables are to be founded.

"I now write to inquire whether you will kindly communicate to me the results of the Greenwich Observations of *Uranus* during the years 1870, 1871, and 1872, to be used in the final equations of condition.

"So far as I have yet compared my final provisional theory with observations, the indications are, that the motion of the planet during the ninety years since its discovery, will be represented within a small fraction of a second of arc, and, in consequence, that no evidence will be found to indicate the action of a trans-Neptunian planet. On this point, however, I cannot yet speak positively. So far as I know, the planet *Neptune* has not yet deviated sensibly from my tables. With the comparison of the Greenwich Observations for 1871 with the *Nautical Almanac*, you are better informed on this point than I am.

"Permit me to congratulate you on setting at rest the question whether the thickness of the objective has any influence on aberration. There could be little doubt of the result after Hoek's experiments; but it is always more satisfactory to test a theory by a crucial experiment—*a posteriori*."

Washington, May 16th, 1872.

Discovery of Minor Planet (121). By Mr. Watson.

(*Extract from a Letter addressed to the Astronomer Royal.*)

I have the pleasure to send you the following observations of a new planet which I have discovered:—

Ann Arbor M.T.		R.A.	Decl.	No. of Comparisons.
	h m s	h m s	° ′ ″	
1872 May 12	12 5 0	16 20 41·20	−18 53 6	1
12	14 13 22	16 20 37·58	−18 53 9·4	5
13	11 13 22	16 19 59·33	−18 52 46·1	6

It shines like a star of the 11th magnitude.

Ann Arbor, May 14, 1872.

Observations of Occultations of Stars by the Moon (with the deduced Equations between the Errors of the Lunar Elements); and of Phenomena of Jupiter's Satellites; made at the Radcliffe Observatory, Oxford, in 1871 *and* 1872.

(*Communicated by the Radcliffe Observer.*)

Occultations.

No.	Day of Observation.	Phenomenon.	Moon's Limb.	Oxford Mean Solar Time.	
	1871.			h m s	
1	Oct. 23	Dis. of τ^1 Aquarii	Dark	8 32 28·0	M.
2	,,	Dis. of τ^2 Aquarii	Dark	9 42 43·1	M. & L.
3	,,	Reapp. of τ^2 Aquarii	Bright	10 53 20·9	M.
4	Nov. 15	Dis. of λ Sagittarii	Dark	5 9 23·4	M. & K.
5	16	Dis. of h^2 Sagittarii	Dark	6 55 46·4	,,
6	18	Dis. of ι Capricorni	Dark	6 57 55·0	,,
7	27	Dis. of ι Tauri	Bright	10 3 20·8	M.
8	,,	Reapp. of ι Tauri	Dark	11 18 23·4	,,
9	Dec. 1	Dis. of γ Cancri	Bright	18 0 58·4	,,
10	,,	Reapp. of γ Cancri	Dark	19 13 9·0	M. & K.
11	20	Dis. of ν Piscium	Dark	12 44 43·9	L. & K.
12	,,	Reapp. of ν Piscium	Bright	13 17 3·6	K.
	1872.				
13	Jan. 23	Dis. of ω Geminorum	Imperfect	6 3 11·4	L.
14	Apr. 13	Reapp. of 5 Geminorum	Bright	7 59 22·5	K.
15	18	Dis. of B.A.C. 3579	Dark	7 51 48·4	M.
16	May 22	Reapp. of ω^2 Scorpii	Bright	10 53 7·6	,,

Oct. 23. All these observations good, and the phenomena instantaneous; the sky was rather cloudy.

Nov. 15. The two observers give the same tenth of a second; the disappearance was very sudden at the beat of the Heliometer clock. No projection on the obscure disk of the Moon, which was distinctly visible. [M.]

Nov. 16. Good; the mean of the observed seconds, 46·0 and 46·8, is taken. The altitude of the star was 2° 21′.

Nov. 18. Good; the star vanished suddenly exactly at the edge of the obscure disk, which was visible; the mean of the observed seconds, 54·9 and 55·1, is taken.

Nov. 27. Disappearance of *ι Tauri*; the observation pretty good, though the disk was considerably obscured by clouds. For the reappearance K gave 24^s·3.

Dec. 1. Disappearance of *γ Cancri*; pretty good. The reappearance was instantaneous. [M.] K gave the same tenth very nearly.

Dec. 20. The two observers agree within a tenth. The Moon tremulous at reappearance.

1872, April 18. Instantaneous.

May 22. The Moon slightly eclipsed, and exactly full; the observation very good. The other observations of this evening were prevented by trees, which were in the way of the Heliometer.

In the following table of the errors of lunar elements resulting from the occultations the Greenwich notation is used, and the ele-

ments of the *Nautical Almanac* uncorrected. All the computations have been made by Mr. Main by the method given in his treatise on *Spherical and Practical Astronomy*.

The observations are referred to by the Nos. of reference given above.

1 $+ 0''.93 = +0.815 \times e + 0.543 \times f - 0.815 \times x - 0.544 \times y - 0.203 \times t$
$- 1.720 \times m - 0.942 \times n.$

2 $+ 7.39 = +0.921 \times e - 0.322 \times f - 0.921 \times x + 0.321 \times y - 0.423 \times t$
$+ 1.592 \times m - 0.942 \times n.$

3 $- 7.08 = -0.747 \times e + 0.641 \times f + 0.747 \times x - 0.641 \times y + 0.444 \times t$
$- 2.884 \times m - 0.941 \times n.$

4 $+ 7.53 = +0.902 \times e + 0.132 \times f - 0.902 \times x - 0.134 \times y - 0.497 \times t$
$+ 0.892 \times m - 0.991 \times n.$

5 $+ 9.21 = +0.845 \times e - 0.376 \times f - 0.845 \times x + 0.374 \times y - 0.496 \times t$
$+ 2.718 \times m - 0.982 \times n.$

6 $+ 3.97 = +0.212 \times e - 0.975 \times f - 0.212 \times x + 0.975 \times y - 0.268 \times t$
$+ 3.366 \times m - 0.960 \times n.$

7 $+ 0.91 = +0.927 \times e - 0.035 \times f - 0.927 \times x + 0.037 \times y - 0.377 \times t$
$- 1.140 \times m - 0.887 \times n.$

8 $- 11.63 = -0.941 \times e + 0.593 \times f + 0.941 \times x - 0.592 \times y + 0.447 \times t$
$- 0.362 \times m - 0.887 \times n.$

9 $+ 9.66 = +0.922 \times e - 0.078 \times f - 0.922 \times x + 0.080 \times y - 0.375 \times t$
$+ 0.929 \times m - 0.890 \times n.$

10 $- 9.75 = -0.775 \times e - 0.545 \times f + 0.775 \times x + 0.546 \times y + 0.405 \times t$
$- 0.276 \times m - 0.890 \times n.$

11 $+ 3.94 = +0.116 \times e - 0.993 \times f - 0.116 \times x + 0.993 \times y - 0.251 \times t$
$+ 2.771 \times m - 0.910 \times n.$

12 $- 10.64 = -0.828 \times e - 0.556 \times f + 0.828 \times x + 0.556 \times y + 0.270 \times t$
$- 0.268 \times m - 0.910 \times n.$

13 $+ 2.66 = +0.427 \times e - 0.882 \times f - 0.427 \times x + 0.883 \times y - 0.258 \times t$
$+ 0.895 \times m - 0.882 \times n.$

14 $- 9.21 = -0.856 \times e + 0.330 \times f + 0.856 \times x - 0.327 \times y + 0.380 \times t$
$- 2.083 \times m - 0.891 \times n.$

15 $+ 5.23 = +0.711 \times e + 0.675 \times f - 0.711 \times x - 0.674 \times y - 0.366 \times t$
$- 1.611 \times m - 0.901 \times n.$

16 $- 7.19 = -0.933 \times e - 0.140 \times f + 0.933 \times x + 0.139 \times y + 0.455 \times t$
$+ 1.079 \times m - 0.983 \times n.$

Phenomena of Jupiter's Satellites.

Day of Obs.	Satellite.	Phenomena.	Oxford Mean Solar Time of Observation.	Greenwich Mean Solar Time from N. A.	Observer.
1872.			h m s	h m s	
Jan. 3	II.	Occ. reap. last contact	9 54 3·4	9 59	K.
8	I.	Ecl. dis. last seen	7 6 18·7	7 10 33·1	„
15	I.	Occ. reap. first app.	11 15 14·0		
	I.	„ bisection	11 16 23·8	11 21	„
	I.	„ last contact	11 17 43·6		
16	III.	Ecl. reap. first seen	11 58 54·9	12 1 47·2	L.
23	I.	Tr. ing. first contact	7 54 1·2	8 4	„
	I.	„ last contact	8 2 4·9		
31	I.	Occ. dis. first contact	6 43 47·9	6 55	„
	I.	„ last contact	6 51 36·6		
	I.	Ecl. reap. first seen	9 31 33·3	9 36 42·7	„
Feb. 2	II.	Tr. ingr. first contact	9 58 13·6	10 6	„
	II.	„ last contact	10 5 22·4		
8	I.	Tr. egr. first app.	8 13 17·9		
	I.	„ bisection	8 14 32·7	8 19	K.
	I.	„ last contact	8 16 2·4		
10	IV.	Ecl. reap. first seen	9 42 29·4	9 50 38·5	„
	IV.	„ usual brightness	9 48 45·4		
17	III.	Tr. ingr. first contact	11 18 13·3		
	III.	„ bisection	11 22 12·7	11 26	„
	III.	„ last contact	11 26 12·1		
20	II.	Tr. egr. last contact	6 43 43·7	6 46	L.
21	I.	Occ. dis. first contact	12 5 44·7	12 13	K.
	I.	„ last contact	12 10 14·0		
23	I.	Occ. dis. first contact	6 31 37·7		
	I.	„ bisection	6 34 47·2	6 39	L.
	I.	„ last contact	6 36 46·9		
Mar. 1	I.	Ecl. reap. first seen	11 39 54·3	11 45 2·0	„
5	II.	Tr. ingr. first contact	8 30 1·0	8 38	„
	II.	„ last contact	8 36 20·0		
	II.	Sh. ingr.	10 41 47·9	10 44	„
	II.	Tr. egr. first app.	11 23 5·1	11 33	„
	II.	„ last contact	11 30 4·0		

Jan. 3. Cloudy.

Jan. 8. The satellite disappeared very close to the edge of the planet, and became very faint before disappearing.

Jan. 16. Overcast, but the observation is satisfactory.

Feb. 21. Very faint; the planet tremulous and ill defined; light clouds.

Day of Obs. 1872.	Satellite.	Phenomena.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
Mar. 6	III.	Occ. dis. first contact	8 6 51·7		
	III.	„ bisection	8 10 36·1	8 16	K.
	III.	„ last contact	8 14 35·4		
	III.	Occ. reap. first app.	11 36 10·6	11 44	„
	III.	„ last contact	11 43 39·4		
	III.	Ecl. dis. last seen	12 32 34·9	12 38 12·4	L.
9	I.	Tr. ingr. first contact	7 30 15·0		
	I.	„ bisection	7 32 44·6	7 37	K.
	I.	„ last contact	7 35 29·1		
	I.	Tr. egr. first app.	9 49 23·1		
	I.	„ bisection	9 51 37·7	9 56	„
	I.	„ last contact	9 54 7·3		
10	I.	Ecl. reap. first seen	8 3 53·5	8 9 7·5	L.
12	II.	Tr. ingr. first contact	10 56 59·5		
	II.	„ bisection	10 59 59·0	11 5	„
	II.	„ last contact	11 4 18·3		
13	III.	Occ. dis. first contact	11 47 37·1	11 55	K.
16	I.	Tr. ingr. first contact	9 20 59·5		
	I.	„ bisection	9 23 59·0	9 27	„
	I.	„ last contact	9 26 28·6		
	I.	Sh. ingr. first app.	10 35 44·6	10 38	L. & K.
	I.	„ well on disk	10 38 4·3		
Apr. 6	II.	Tr. egr. first app.	10 45 20·4		
	II.	„ bisection	10 47 35·0	10 54	K.
	II.	„ last contact	10 50 4·6		
8	I.	Tr. ingr. first contact	9 28 13·9		
	I.	„ bisection	9 30 58·4	9 35	„
	I.	„ last contact	9 33 43·0		
	I.	Tr. egr. first app.	11 50 20·8	11 54	„
	I.	„ clear of disk	11 56 19·8		
	IV.	Tr. egr. bisection	12 14 16·9	11 56	„
	IV.	„ last contact	12 20 15·9		
9	I.	Ecl. reap. first seen	10 14 26·0	10 19 34·8	L.
	I.	„ full brightness	10 15 36·9		

Mar. 6. Moisture on the object-glass, which rendered the eclipse of the third satellite less satisfactory than it would otherwise have been. It was about 40° in entering the shadow of the planet.

Mar. 13. Cloudy.

Mar. 16. Cloudy at the ingress of the first satellite.

Apr. 8. The observations of the egress of the first satellite not very good. as *Jupiter* was seen through the branches of a tree. The "bisection" of fourth satellite not quite so satisfactory as the last contact, the satellite being very faint, the planet seen through the branches of a tree.

Day of Obs. 1872.	Satellite.	Phenomena.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
Apr. 11	III.	Ecl. reap. first seen	12 0 52·0	12 4 53·5	K.
	III.	„ usual brightness	12 3 5·6		
13	II.	Tr. ingr. first contact	10 26 46·3	10 37	L.
	II.	„ last contact	10 34 16·1		
15	II.	Ecl. reap. first seen	10 55 40·9	11 1 5·9	K.
	II.	„ full brightness	10 56 43·2		
	I.	Tr. ingr. first contact	11 22 6·1	11 30	„
	I.	„ bisection	11 24 35·7		
	I.	„ last contact	11 27 35·2		
16	I.	Occ. dis. first contact	8 30 0·2	8 40	L.
	I.	„ last contact	8 36 9·2		
17	IV.	Ecl. reap. first seen	10 14 18·6	10 22 32·6	„
	IV.	„ fully seen	10 19 27·8		
	IV.	„ full brightness	10 22 27·3		
23	I.	Occ. dis. first contact	10 28 48·1	10 37	„
	I.	„ bisection	10 31 47·6		
	I.	„ last contact	10 34 47·1		
May 1	I.	Tr. ingr. first contact	9 45 52·6	9 52	K.
	I.	„ bisection	9 48 37·1		
	I.	„ last contact	9 51 51·6		

Apr. 15. The same remark applies to the ingress of I. as on April 8.

The initials M., L., and K., are those of Mr. Main, Mr. Lucas, and Mr. Keating.

The observations were generally made either with the Heliometer or with the 10-foot telescope, or with both. The 42-inch Dollond was used on November 15 and 16 by K. and on December 20 by L., for the occultations of stars.

On Photographic Irradiation in over-exposed Plates. By Lord Lindsay and Mr. A. Cowper Ranyard.

The most cursory observer of any of the recent corona photographs must have remarked the apparent eating-in of the prominences over the limb of the dark Moon. A more careful examination of the photographs shows that the whole limb of the Moon is more or less eaten into, and that the indentations under the prominences are only exaggerations of a phenomenon which is present at all parts of the limb, but which varies in intensity according as the dark limb of the Moon is projected on a brighter or less luminous background.

In all over-exposed photographs of luminous objects upon a dark background, the brighter parts of the picture are found to be surrounded by a nebulous haze or border of light, which

increases the diameter of the image formed by the luminous objects at the expense of those which are less luminous.

This nebulous haze has often been spoken of as "the extension of the chemical action," but without begging the question of its cause, we propose to speak of it as photographic irradiation. It has been found to vary with the time of exposure, and the relative brightness of the object and its background.

On examining the effects of photographic irradiation in a decidedly over-exposed picture, it will be seen that the nebulous fringes round luminous objects are distinctly divided into two parts—an inner and very marked border of light, following the contour of the luminous objects, and an outer and much less definite haze, thus:—

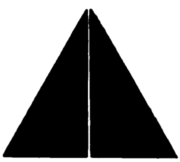


Fig. A.

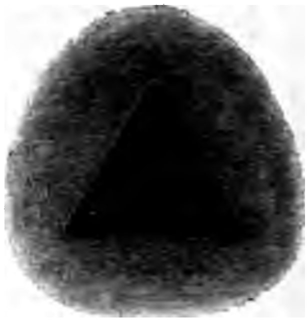


Fig. B.

where fig. A represents a normal photograph, and fig. B a decidedly over-exposed plate from the same object.

The inner border of light fades gradually from the inside outwards, and it is very difficult, and indeed impossible, to tell where the true image of the luminous object ends, and its photographic irradiation begins. While, on the other hand, the boundary between the outer and inner fringes (or halos) of irradiation is more definitely marked, although it would be difficult to say with any absolute precision, at what point the inner fringe terminates.

Our first experiments were devised in order to test whether reflections from the back surface of the plate played any part in the production of the fringes; for this purpose plates of ebonite and the so-called non-actinic yellow glass were prepared.

In the over-exposed photographs taken on ebonite, it was found that the outer haze had entirely disappeared; while in the photographs taken on plates of yellow glass the outer haze is still distinctly to be traced, though it is much fainter than on an ordinary white glass plate with the same exposure.

By placing a piece of wetted black paper at the back of an unground plate the outer haze may be greatly reduced, while it was found that by grinding both the back and the front surfaces

of a yellow glass-plate, and covering the back with a coating of black varnish, the outer haze may be rendered quite imperceptible, while, however, the inner border of irradiation still remains as before.

From these experiments we may conclude that the outer haze is produced by reflections from the back of the plate; and the action of the wetted black paper in reducing the outer irradiation, may be explained by the consideration that the change of refractive index in passing from the glass to the film of water behind, is much less than in passing from glass into air. There is consequently less reflection at the back surface of the plate; most of the light immerges into the film of water, and is then absorbed by the black paper.

Fig. C represents a photograph taken upon a yellow glass plate, with a backing of wet black paper, but otherwise exposed under similar conditions to the photograph represented in fig. B.

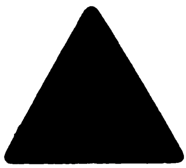


Fig. c.

The outer irradiation halo may therefore be entirely avoided for the future in any corona or other necessarily over-exposed photographs by the use of the opaque plates. If, however, it is considered important that the negatives should be capable of being copied by transmitted light, the outer halo may be still to a great extent avoided by the use of yellow glass plates with a backing of wet black paper or black varnish.

Secondly, as to the inner and more definitely-marked irradiation-edge which remained and seemed to be unaffected by the precautions that had served to rid us of the outer halo. Since the inner fringe was equally to be found on an opaque, and on a transparent plate, we felt ourselves justified in seeking for its cause in front of the first or upper surface of the prepared plate; that is, it must be referred either to some action taking place within the thickness of the collodion, or to the optical imperfections of the instrument.

In order to determine whether the scene of action lay within the thickness of the collodion, we placed an ivory ruler with a bevelled edge in immediate contact with the collodion film. The plate with the ruler upon it was then exposed within the camera, so that the image of an incandescent platinum wire fell partly upon the collodion film, and partly upon the ivory ruler. If the scene of action lay within the collodion film, we might expect the inner irradiation fringe to extend itself under the edge of the ruler, while if it were due to the optical imperfections of the lens, the image of the wire would be cut off sharply by the edge of the ruler.

On removing the plate from the camera, and before the ruler was shifted from its place on the collodion, the whole was exposed for a few seconds to the action of the light from a gas-burner, in order that the position occupied by the edge of the

ruler might be faintly printed upon the collodion film. On developing the plate, it was found that the image of the wire was sharply cut off at the place occupied by the edge of the ruler, as in fig. D.

Fig. D.

The very faint action extending inwards under the ruler being evidently due to the want of perfect opacity in the ivory, it seems, therefore, to be clearly proved that the inner irradiation-edge is not caused by any chemical or other action taking place within the thickness of the collodion. But must be referred to the optical imperfections of the instrument which throws the image upon the collodion film.

It is instructive to remark that the photographic image of the wire is not cut off by an absolutely straight line at the edge of the ruler, but it is slightly convex, and is separated from the faint action which has apparently taken place through the ruler by a very narrow bright line, which appears to indicate the presence of a small capillary film of liquid along the edges of the ruler forming a minute cylindrical lens. At the point where the collodion was acted upon by the light, the minute cylindrical lens appears to have been interfered with, and depressed inwards towards the ruler, we may therefore conclude that the collodion film is slightly swelled or thickened by the action of the light upon it.*

The cause of the inner irradiation-edge seems to be that every point of a luminous object is not represented by a simple point of light in the luminous image; in other words, the circle of least diffusion of any pencil is a curve of sensible area, of which the central and most intense portions imprint themselves first upon the collodion.

Some further experiments were made in order to test whether the size of the circles of least diffusion was chiefly owing to chromatic aberration (in which case the difficulty might be got rid of by the use of reflectors); for this purpose a bath of solution of sulphate of copper was placed in front of a gas-burner, and the triangular diaphragm shown in fig. A was then placed

* We are at present unable to find any explanation of the slight apparent thickening of the end of the image of the wire where it abuts upon the ruler, but the same thickening is to be found in all the plates. It may be well to remark that it appears evident from slight indication in the negatives which it would be difficult to render in a woodcut, that the true edge of the ruler coincides with the inner side of the white line (or with the side away from the image of the wire). The convexity of the end of the image of the wire cannot, therefore, be regarded as indicating even a slight chemical encroachment. The slightly tapered appearance of the other end of the image of the wire is due to the fact that the platinum incandescent wire is cooled at its points of contact with the thick copper wires of the circuit.

between the sulphate of copper bath and the lens of the camera, but the blue screen thus formed seemed to have very little effect in altering the breadth of the irradiation-fringe, only slightly retarding the rate of its formation; a similar result was obtained on placing a piece of yellow glass in front of the diaphragm; in this case, however, the formation of the fringe was still further retarded.

Photographers have long known that by making use of stops they can obtain a much sharper image. By way of experiment, we cut off the edges of the lens with a circular stop, and found that the inner irradiation fringe was thus greatly decreased. It seems, therefore, fair to argue that the aberration of oblique pencils exceeds in magnitude the other disturbing causes, and that it will be well, in making preparations for the photographic observation of the Transit of *Venus*, to avoid as much as possible all oblique pencils.

We would, therefore, place our photographic plates in the primary focus, and thus avoid the necessarily deep curves of any arrangement of lenses which may be used for enlarging the image. Whether it would be best to make use of a reflector or a refractor, remains to be settled by further experiment, but our present experiences would lead us to vote in favour of the reflector.

We cannot conclude without returning our best thanks to Mr. H. Davis, who has rendered us the most willing assistance in carrying out all of the foregoing experiments.

The Aurora of February 4th, 1872. By the Rev. J. Slatter.

A desire having been expressed by Mr. Finlayson in the April Number of the *Monthly Notices* for any observation of this aurora, I forward to you the entry I made of it.

Feb. 4, $7^h\ 20^m$. A fine aurora with much rosy light, but hidden by low fleecy clouds, with a slight drizzling rain. At the time indicated, there was a break in the clouds which showed the dome-formation, and suggested a corona either formed and passed, or about to form, exactly over *Aldebaran*, by about 5°, which was then in azimuth almost on the magnetic meridian.

My place of observation is in N. lat. 51° 30′ 15″; long. W. 1° 1′ 15″.

On Oct. 24, 1870, I obtained a simultaneous observation of the corona with Greenwich, viz. at $8^h\ 20^m$, G.M.T. At this station it exactly covered γ *Pegasi;* at Greenwich it covered a spot midway between β and η *Pegasi*, which may be assumed to have been R.A. $22^h\ 47^m\ 13^s$, N.P.D. 61° 32′.

From these data it would appear that the height of the corona was nearly 118 miles in the zenith of a spot in the Channel about thirty miles north of Havre. But this depends entirely on the assumption that it was the same phenomenon (*i.e.* identical and not only apparent) which was observed at both places. The

only other observation I ever made, in which I was satisfied by ocular demonstration of the identity of the phenomenon, was the height of an auroral arch with a base of nineteen miles which gave a height of only 15,000 feet. With regard to the intensity of the light on the occasion just referred to, viz. Oct. 24, 1870, it may be worth recording that, great as was the amount of light pervading the atmosphere, no portion of it, not even the corona, was capable of being seen by reflexion in the centre of the river.

Streatley Vicarage, June 4th, 1872.

Observations of the Solar Prominences, from Jan. 1 to April 29. By Father Secchi, Director of the Roman Observatory.

Father Secchi communicates to the Paris Academy his observations of the prominences during the four solar rotations ending April 29, thus completing, with the observations already recorded (see *Monthly Notices* for March), an entire year of observations. In this period there were fifty-nine days of complete observations.

During this interval, the law was confirmed, which gave a maximum of prominences in the region of spots, and a minimum of the equator. But the relative maximum in the polar zones was scarcely sensible.

"The general absence of polar prominences," proceeds Secchi, "during the interval was remarkable." They were replaced by sensible elevations of the chromatosphere. Counting only as prominences those whose height reached or surpassed forty seconds, prominences were found to be very rare.

"The aspect of the granulations, and of the brighter bands around the polar zones, corresponded with this absence of polar prominences; as the former could scarcely be recognised, while last year they were very visible.

"The intensity and number of the faculæ are also diminished.

"Dividing the prominences into three classes, according to their direction with respect to the poles, the following numbers are obtained:—

Indifferent	398
Directed towards the poles.. ..	342
Directed towards the equator ..	67
Total	807

"In the observations, especially the later ones, great attention has been paid to the direction of the threads or hairs of the chromatosphere, and the results obtained have been the following:—

"In general, in mean latitudes, the threads are also directed towards the poles, but there are numerous exceptions, especially in the neighbourhood of prominences, spots, and granulations. At the equator and poles, there are no constant rules.

"The variability of this inclination is often astonishing at one and the same place. It even appears to change during the observation, in such sort that I have often been the victim of an illusion; but a sustained attention, in the clearer days, has proved that this variability is real. One would be tempted to compare this diffusion to a species of very variable electric sparkling, rather than to an emission of real matter: but it would be premature to pronounce, and we do not pretend here to prejudge the true interpretation. It is necessary to mention that sometimes, when the hairs are very fine, they appear divided into two opposite directions, with respect to the slice of the chromatosphere limited by the border of the slit. All the circumstances show how difficult these observations are, and how much yet remains to be cleared up.

"One of the most remarkable peculiarities attracting attention in these researches, is the fixity of the chromatospheric layer and its features, amidst the apparent agitation of the solar edge. It is truly astonishing to see the streak adorned with fringings and hairs of the utmost delicacy, resting immovable in the middle of the disturbance agitating the light of the Sun's limb. This proves that the agitation is factitious, that it is due to our atmosphere, and does not displace the direct image of the Sun. The effect is somewhat like that produced during the observation of stars near the horizon, where one sees the black lines remaining immovable in the midst of the incessant waves seen to traverse the spectrum. This leads me to the inquiry whether one would not gain considerably in precision in taking the passage of the solar disk with the help of a spectroscope: observations have shown me that there would be a sensible advantage in this course; but to render this system perfectly practicable, it would be necessary to fulfil certain conditions as to details, which I have not yet been able to accomplish.

"I have inquired whether the direction of the hairs of the chromatosphere might not be influenced by this agitation, but I have convinced myself that it is not so influenced to any extent. These hairs, which seem to us so fine, are veritable flames on the Sun, and it is natural that our atmosphere should have no influence upon them.

"It is seen, by all these results, how necessary it is that the observations of the prominences should be made in a continuous manner; and at least during one full solar spot period, in order that we may be enabled to compare the vicissitudes of the chromatosphere with those of the photosphere. The three last months have shown a medium activity of spots and prominences, in reality they seem to be reviving. I occupy myself, at present, in discussing the relations of the two phenomena, availing myself of the drawings which I have made during the year. The comparison of these drawings with the fine photographs of M. Capello, has convinced me that our drawings, without attaining the perfection

of photographic images, are nevertheless capable of being sufficiently useful to the science of the day.

"When the sky is not clear, the chromatosphere can still be observed, if the veil is simply caused by mists formed of water globules; one can see nothing if the veil is formed of small ice-crystals. With the aqueous veil, the bright line of the chromatosphere seems bordered by two very strong dark lines, which seem to detach it from the Sun; these lines are evidently due to the aqueous vapour in our atmosphere. The fact that the length of these lines increases with the density of the veil leaves no doubt as to their origin.

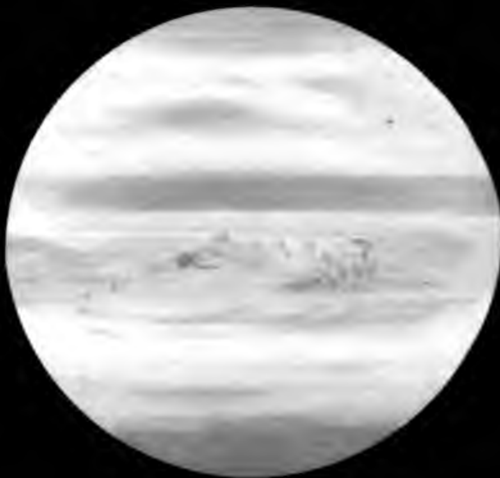
"Let me be permitted to return to the structure of the corona of which I spoke in my last communication, to remove objections which may be raised.

"The curvilinear rays lead us to think that there is a certain circulation there; but ought we to suppose this circulation to proceed like that produced on a calm planet warmed from outside? Not at all, unquestionably. We have in the Sun an element which deserves special recognition; it is not merely the ascensional motion of masses warmed below, but another cause, perhaps more potent, the material projection from within outwards, which one must also consider. Neglecting the resistance of the medium in which the solar emissions take place, we should have, taking the evaluations of most distinguished men of science, initial velocities capable of projecting matter beyond the sphere of solar attraction. No doubt these velocities are exceptional, and are attained only during the epochs of greatest activity. But if the ordinary velocities cannot project matter to such distances, they may well carry it as far as the limits of the visible corona, after which it would fall back upon the Sun, after having been cooled by dilatation and by radiation in space These masses, in rising, feel the influence of the layers they traverse, which should modify their direction, and bend them into curvilinear threads if these layers themselves have a circulation of their own.

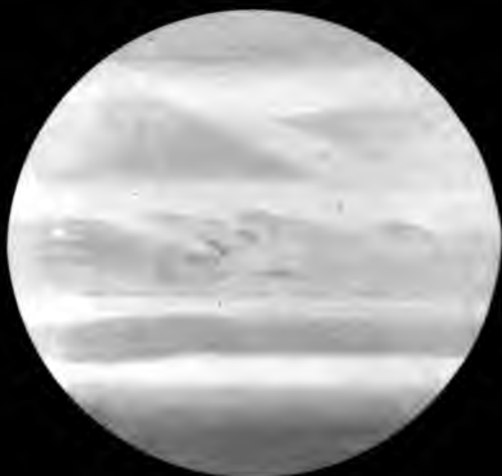
"According to these considerations, the solar corona would not be merely formed of a layer circulating quietly, but it would be broken by jets proceeding from the body of the Sun; its constitution could not be assimilated to that of an envelope in dynamical equilibrium, circulating according to the laws of rotation and of the difference of densities produced by lower heat.

"These considerations seem to me well fitted to enable us to appreciate and explain the movements and the curious curves which are presented in Lord Lindsay's photographs, as well as in the drawings by M. Liais and other observers."

Father Secchi's paper is accompanied by a series of tables resembling those which accompanied his former paper. As the evidence they afford accords in much the same way with his inferences, it does not seem necessary to reproduce these tables.



JANY 14TH 7 P.M. 1872.



MAY 5TH 9.15 P.M. 1872.

JOHN BROWNING, DEL.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXII. *Supplementary Notice.* No. 9.

On some Observations of Jupiter in 1871–72.

By John Browning.

From the drawings of *Jupiter* which I have made since December 1871, I have selected only four to bring before the notice of the Society: these drawings were made on the following dates: No. 1, on Dec. 4th, 1871, at midnight; No. 2, on Jan. 14th, 1872, 7 P.M.; No. 3, on March 3rd, 1872, at 8.45 P.M.; No. 4, on May 5th, 1872, at 9.15 P.M.* Many of the drawings made during the winter months are somewhat deficient in detail as compared with those made recently; but this, I do not doubt, is principally the result of the bad definition due to very unsteady air, as the first drawing mentioned, that made on Dec. 4th, contains nearly as much detail as those made recently; the markings are, however, so complex that they defy all attempts to draw them correctly. I had some tolerably good views of the planet earlier than December last year; the tawny colour of the equatoreal belt seemed to me stronger than I had previously seen it; as, however, a well-known observer, who had previously made colour observations of *Jupiter*, did not agree with me, I preferred making a continuous series of observations before giving a decided opinion on the subject. Subsequent observations have convinced me that my conclusion was correct, and I have since been fortified in this opinion by the statement of our late President, Mr. Lassell. On every occasion when the definition has been good, I have been able to make out light markings flecking the

* The four drawings were exhibited at the meeting in June. Nos. 2 and 4 are reproduced on a reduced scale in the accompanying illustrations.

tawny-coloured surface of the equatoreal belt. The drawings I have made do not show any remarkably symmetrical forms; in several instances I had drawn nearly oval markings or turreted forms on the southern edge of the equatorial belt, but using the highest powers the night would bear, and watching for fitful intervals of the best definition, I always found these exceedingly regular forms were incorrect, and had to modify them: for this reason I rather distrust the drawing made on the 3rd of March. The markings on the dark belt, south of the equator, present forms similar to those we see at times in white cumulus clouds previous to a thunder-storm. I believe that could I have obtained clearer views of the planet, I should not have left these markings quite so regular as they now appear in the drawing. When favourably seen, such markings usually terminate in points or rugged edges towards the W. The light belts frequently incline at a considerable angle from the poles towards the equator of the planet. The belt shown on the drawing No. 4, made on May 5th, at 9.15 P.M., G. M. T., makes an angle of nearly 25° with the equatorial belt; a number of shorter markings, also, at about the same angle, stretch some distance into the tawny-coloured equatorial belt; these I at first drew of a turreted form, and only very close examination convinced me that they had the inclination I have now given to them. The darker belts have varied from dark warm or cool grey, to purple madder, or madder brown; but towards the poles of the planet the belts have been usually bluish grey, while close to the poles the blue colour has been very decided. During the month of May the colour of the equatorial belt had seemed fainter, but this is probably due to the planet being less favourably situated for observation.

I have made several observations of the spectrum of the planet, but though I have 12 inches of aperture, I do not find this sufficient to enable me to see more than the dark absorption bands in the red portion of the spectrum, with which most observers are now probably familiar. Though I cannot make out any differences in the appearance of the spectrum sufficiently marked to enable me to draw or describe them, yet I strongly suspect a change in the spectrum, which a larger aperture, giving more light, would enable me to bring out.

On the Masses of the Planets and the Parallax of the Sun.

By M. Le Verrier.*

The exact determination of the angle π, or the solar parallax is a matter of great interest to astronomers; it is the maximum angle under which an observer, supposed to be placed at the Sun's centre, would see the radius of the terrestrial globe.

* Translated from the *Comptes Rendus* for 1872, July 22, by W. T. Lynn, B.A., F.R.A.S.

The solar parallax being known in sexagesimal seconds, it is only necessary to find how many times it is contained in the number 206265, to conclude from it the Sun's distance from the Earth, referred to the radius of our globe as unity.

Laplace, in common with the other French astronomers of his epoch, adopted in the *Mécanique Céleste* the parallax 8″·813, derived from the transits of *Venus* observed in 1761 and 1769. From this it had been concluded that the Sun's distance from the Earth was 23405 radii of the Earth.

Afterwards Encke, having re-discussed the observations of those transits, determined that the parallax was only 8″·578, and that the Sun's distance ought consequently to be regarded as 24046 terrestrial radii.

It has been perceived that the change made by the Berlin astronomer in Laplace's number was not a happy one, and that, instead of taking anything away from it, the value 8″·813, attributed to the solar parallax in the *Mécanique Céleste*, should rather have been increased by some hundredths of a second.

Whether the Sun's distance from the Earth is greater or less by a small fraction of its amount, is not, considered in itself, a matter of such great interest. But the knowledge of the solar parallax is useful in many astronomical calculations: in particular, it gives the means of determining the value of the Earth's mass, and of taking account of its action upon the Moon, by introducing it into the calculations of the *Mécanique Céleste.*

Newton has furnished for this purpose a method which comes, in fact, following the *Mécanique Céleste*, to the employment of the formula

$$m = 4\cdot4320 \left(\frac{\pi}{1000}\right)^3,$$

m being the value of the Earth's mass referred to that of the Sun as unity.

If we admit 8″·813 as the value of π as the parallax, we find that the ratio of the mass of the Earth to that of the Sun is $\frac{1}{326630}$. The reduction of $\frac{1}{38}$th, proposed by Encke in the value of the parallax, would lead us to attribute to the Earth a mass less by $\frac{1}{13}$th than that which we have just given.

The preceding formula may also be solved with respect to π, which gives

$$\pi = 608\cdot79 \sqrt[3]{m}.$$

This shows that if we could determine directly the value m of the Earth's mass, we could deduce the solar parallax therefrom.

If we set out by determining the parallax, the relative error that may be committed becomes threefold in the deduction of the value of the mass of the Earth.

If, on the other hand, we commence by determining the Earth's mass, the relative error in the deduction of the parallax becomes three times smaller. An error of $\frac{1}{300}$ in the value adopted for the Earth's mass corresponds to an error of $\frac{1}{900}$ in

the value concluded for the parallax, that is to say, to very nearly 0″·01.

The attraction of the Earth produces sensible inequalities in the motions of *Venus* and *Mars*. By determining the amplitude of these by observations, it is possible to conclude directly from them the mass of our planet. It may be worth while to examine to what degree of exactitude we can arrive by this means.

To increase the precision of the result, we may unite the consideration of the periodic inequalities with that of the secular inequalities.

The elements of the orbits of *Venus* and *Mars* undergo variations called secular, which increase from year to year, from century to century, and at last acquire very large values, which renders them very suitable for resolving the question with which we are now occupied. It may even be said, *à priori*, that their consideration offers a method which ought, *in the course of time*, to equal in precision that which may be expected from the direct measure of the solar parallax, and at last even to exceed it. Let us inquire whether this epoch may not have now arrived.

At the time of the celebrated transits of *Venus* over the Sun in 1761 and 1769, Bradley had, ten years before, commenced the series of meridian observations which Maskelyne, Pond, and Airy have continued to the present time without interruption. By establishing at Greenwich the instrument invented fifty years before by Roemer, Bradley secured for England the possession of one of the fundamental bases of exact astronomy. This is what might have been done for France from the commencement of the eighteenth century.

The labours of Bradley gave the means of determining with accuracy the elements of the orbits of *Venus* and *Mars* for the epoch 1755. But nothing else of a precise nature was possessed in the way of observations, and it was necessary that long years should elapse before it was possible to demonstrate and measure the *variations* of the elements of the orbits. One hundred and twenty-one years have now passed away since Bradley's observations were made.

The variations of the excentricities, of the perihelia, of the inclinations, and of the nodes of *Venus* and *Mars*, may all be made to contribute to the result. Let us pause at the most important, that of the perihelion of *Mars*. The planet is in a position to be observed with great accuracy at its oppositions; and, moreover, we possess a triple simultaneous observation made on the 1st October, 1672, by Picard, Roemer, and Richer, which extends to two hundred years the period of which we are able to avail ourselves.

The attraction of the Earth changes by 50″ in a century the heliocentric position of *Mars'* perihelion.

When *Mars* is actually in opposition, this change subtends at the Earth an angle of 185″.

The variation in two centuries and for the position of the

planet *Mars* on the 1st of October, 1672, is, seen from the Earth, 294″ of longitude.

The discussion of the great series of meridional observations leads us to estimate that we can generally deduce from them, within a second, the value of the geocentric errors resulting from the consideration of the whole mass of observations. We may, therefore, consider that the epoch has arrived when the direct determination of the Earth's mass can be obtained, by the employment of the secular variations of the elements of the orbits of the planets, with a precision at least equal to that afforded by the direct observation of the Sun's parallax.

Let

$$(1)\quad \begin{cases} m = 0{,}000000333\,(1 + \nu) & \ldots \text{ Mercury,} \\ m_1 = 0{,}000002489\,(1 + \nu_1) & \ldots \text{ Venus,} \\ m_2 = 0{,}000002817\,(1 + \nu_2) & \ldots \text{ The Earth,} \\ m_3 = 0{,}000000373\,(1 + \nu_3) & \ldots \text{ Mars,} \\ m_4 = 0{,}000952381\,(1 + \nu_4) & \ldots \text{ Jupiter,} \end{cases}$$

represent the real masses of *Mercury*, *Venus*, the *Earth*, *Mars*, and *Jupiter*; the numerical co-efficients representing the values of the masses which in the *Annales* have been used as starting-points, and ν, ν_1, ν_2, ν_3, ν_4, being the indeterminate quantities to be investigated by making them satisfy the observations.

The mass of *Mercury* results from the perturbations which that planet produces in the motions of *Venus*. The discussion of the observations made at Greenwich from 1751 to 1761, and from 1766 to 1830, furnishes as equation of condition:—

$$(2)\qquad 18''{\cdot}2\,\nu + 30''{\cdot}1\,\nu_1 + 35''{\cdot}35\,\nu_2 + 3''{\cdot}55 = 0.$$

Venus changes the position of the plane of the ecliptic. In order to satisfy the observed motion, we have the equation:—

$$(3)\qquad + 0''{\cdot}53\,\nu + 28''{\cdot}88\,\nu_1 + 0''{\cdot}83\,\nu_2 + 1''{\cdot}72 = 0.$$

The mass of *Mars* results from the discussion of the meridional observations of the Sun. This gives

$$(4)\qquad \nu_3 - 0''{\cdot}016\,\nu + 0''{\cdot}484\,\nu_1 + 0''{\cdot}071 = 0.$$

The observations of the fourth satellite of *Jupiter* and the perturbations of the system of small planets furnish the value

$$(5)\qquad \nu_4 = + 0''{\cdot}0012.$$

The mass of the Earth enters advantageously for its determination in three relations.

The first (A) of these relations * results from the latitudes of

* In the original, a misprint 'rotations.'

Venus at the moments of the transits in 1761 and 1769. It is independent of the uncertainty in the apparent diameter of the Sun, and possesses an extreme precision :—

$$\text{(A)} \qquad -0''{\cdot}53\,\nu + 24''{\cdot}6\,\nu_1 + 32''{\cdot}8\,\nu_2 - 1''{\cdot}86 = 0.$$

The second (B) is furnished by the discussion of the meridional observations of *Venus*, in an interval of one hundred and six years :—

$$\text{(B)} \qquad -0''{\cdot}76\,\nu + 24''{\cdot}6\,\nu_1 + 32''{\cdot}9\,\nu_2 - 2''{\cdot}00 = 0.$$

This agrees very nearly with the first (A), which is a proof of the accuracy, both of the observations themselves and of their discussion.

Lastly, we will deduce the third or (C) from the observations of the 1st of October, 1672. Richer at Cayenne, Picard near Beaufort, and Roemer at Paris, all compared *Mars* with the star ψ *Aquarii*, which was then occulted by the planet. Their separate comparisons differ respectively only by the very small quantities $0''{\cdot}5$, $0''{\cdot}8$, and $0''{\cdot}3$. On the other hand, Bradley has left us thirty-two meridional observations of the three stars ψ, ψ^1, and ψ^2 *Aquarii*; we have discussed these with great care, and here also there is every guarantee of accuracy :—

$$\text{(C)} \qquad 225''{\cdot}3\,\nu_2 + 29''{\cdot}5\,\nu_1 + 1398''\,\nu_4 - 21''{\cdot}86 = 0.$$

If, making use of the equations of condition (2) (3), (4) and (5), we eliminate the indeterminate quantities ν, ν_1, ν_2, ν_4, we derive from this process, by means of each of the equations of condition (A), (B), (C), treated separately, the following values for the ratio $1 + \nu_2$ of the new mass of the Earth to that which was adopted as the starting-point; also for the value of the solar parallax deduced therefrom :

	$1+\nu_2$	Deduced Solar Parallax.
(A)	1,0917	$8''{\cdot}853$
(B)	1,0937	8·859
(C)	1,0965	8·866

It is well known that L. Foucault has found, by direct measurement of the velocity of light, adopting the constant of aberration determined by Struve, the number for π, $8''{\cdot}86$.

It appears from these results that the epoch has really now arrived when the value of the mass of the Earth, which ought to be introduced into the calculations of the *Mécanique Céleste*, can and should be derived directly from the motions of the planets, and no longer though the intervention of the solar parallax.

But it may be asked, does it follow that it is legitimate to conclude from the former by a reciprocal process the value of the solar parallax itself? Undoubtedly it would be, if we were certain that the celestial bodies which we take into account were

the only ones that could come into consideration. But, whatever reasons we may at present have for regarding the aggregate mass of the minor planets as extremely small, it is nevertheless possible that their attraction, continuing to accumulate in the course of time, may at last become sensible; and it is proper to ask whether a proof of this can be found in the difference between the values of the parallax determined from the discussion of the movements of the celestial bodies, from the measurement of the velocity of light and of aberration, and lastly, from the observation of the transit of *Venus* over the Sun's disc. It is not easy to answer this question; it depends upon the magnitude of the unknown influence, the determination of which is itself the matter under consideration.

If the discussion of the motions of the celestial bodies should give a value of the parallax larger by $\frac{1}{10}$th of a second than that which is deduced from the velocity of light, and than that which may be deduced from the transits of *Venus*, we might attribute this difference to the attraction of the minor planets, and so measure the amount of their influence. But if this influence is represented by a result corresponding only to $\frac{1}{100}$th of a second in the value of the solar parallax, all that could be concluded would be that the total mass of the minor planets is excessively small, but that its actual amount could not be measured.

Such may, in fact, be the case. The attraction of the minor planets must certainly, if sensible at all, be much more so in its effect upon the planet *Mars* than upon *Venus*. And since the discussion of the observation of *Venus* leads us to the same result, as that of the observations of *Mars*, and since also this result is the same as that derived by Foucault from the velocity of light, it appears very probable that the attraction of the minor planets amounts up to the present time to a quantity which may be neglected.

But it is necessary here to remark that as the angular errors introduced by the perturbations in the positions of the planets *Mercury*, *Venus*, the Earth, and *Mars*, go on constantly increasing, in the course of time an epoch must arrive when it will no longer be possible to put all these quantities in accord with each other, without perhaps introducing new forces. This is even now certainly the case with respect to regions situated between *Mercury* and the Sun. Thus there is a considerable quantity of matter which has hitherto escaped our regular investigations.

These things being so, it seems that Astronomy ought to enter upon a path somewhat new; and I would venture to ask the Academy to interest itself in a number of pieces of work which have now become necessary.

(1.) In the first place, it is necessary to open what we will call, the better to express clearly our idea, *the taking account of celestial masses.** For this purpose, going back into the past, we should inquire with care the circumstances under which per-

* In the original, 'Le compte des matières célestes.'

[illegible] peculiar to each and with a planet have produced remarkable perturbations, and examine the equation of condition which results from this for the determination of its mass. In doing so, use ought to be made of all the past observations made at different observatories.

At the same time the most favourable future circumstances for the determination of masses might be investigated, and made known to astronomers beforehand, so as to secure obtaining the necessary observations.

Thus would result a mass of conditions, the fruits of which would accumulate daily, and lead to the most important results.

(2.) We would ask physicists again to take up the direct measurement of the velocity of light. The Academy would without doubt obtain this from M. Fizeau.

(3.) The determination of the constant of aberration ought to be an object of attention to astronomers; it would be very interesting, at a time when the constant determined by M. Struve plays so special a part, to be possessed of the grounds of the opinion of that eminent astronomer on the degree of accuracy which he is sure of having attained.

(4.) Lastly, the determination of the solar parallax by means of the transits of Venus still retains all its interest, but conditionally on its being made with exceptional precision, so that the astronomer may be able to answer for it with an accuracy corresponding to $\frac{1}{100}$th of a second of arc, or about the $\frac{1}{800}$th part of the total value of the parallax.

With this limit of extreme precision the labour becomes one of the most delicate works of art, and one which ought only to be confided to men who have given special guarantees of their devotion to the enterprise, and who have themselves resolved to carry out the observations. We think the Academy should name them without delay, leaving to them the whole of the responsibility, but also providing them with every assistance in procuring the means of execution.

After the reading of this paper, some remarks were made by M. Fizeau and M. D'Abbadie on the question of the desirability of re-determining the velocity of light by the method of Foucault. M. D'Abbadie said that Foucault's result was, in fact, a happy coincidence. The base selected by that ingenious physicist was so small (only two or three metres, in fact, in length) that any error made in measuring it was enormously increased in the determination of the resulting velocity. But M. Fizeau had previously invented a much better method which rendered it possible, by selecting suitable stations, to take a base of more than 100 kilometres, which geodeters can measure within a decimeter. In making a new determination, M. D'Abbadie thought it extremely desirable to use this method in preference to the much less exact one of Foucault.

Observations made during the Eclipse of June 6, 1872. By A. Nursing Row, Esq.

(*From a Letter to Dr. Huggins.*)

I have the pleasure to enclose a copy of the paper printed by me, with the particulars of the solar eclipse of the 6th instant, for the use of my friends and the community, European and native, in the neighbourhood of Vizagapatam. I am sorry to say, that we were denuded *in toto* of the means to observe the first contact and the greatest phase, the clouds were so dense and threatening rain.

We were in despair until ten minutes to the last contact time, but then the Sun made his appearance from an aperture in the mass of clouds around him, and continued to do so until the last contact was observed most satisfactorily. The error of the prediction for the last contact, according to the observation, was +7 seconds.

The enclosed statement contains the observations of the barometer, the solar radiation, the humidity, &c. for every ten minutes during the eclipse.

The solar radiation observations were made with the black-bulb thermometer that was used the last solar eclipse-time—description of which was given in my last report,* 13th Dec. 1871.

The height of the instrument from the ground, and the character of the ground, are one and the same this time and last time.

The magnitude of the present eclipse is ·927, that of the last eclipse was ·629. With so much sheltering from the Sun's rays by the opaque body of the Moon at the greatest phase of the eclipse, and with those well-known rain-clouds—nimbus covering the whole sky, which will, no doubt, present an obstruction to the free passage of half the solar radiation — I surmised that the radiation thermometer would decrease more than it did at the greatest phase of the last solar eclipse, but it decreased 1°·5 less than it did last time. In addition to the vicissitudes of the season, which create more or less variation of the temperature, there may be various other causes for it.

In this discussion, I think, the altitude of the Sun must be considered. On the date of the last solar eclipse, 12th December, the apparent time of Sun-rising in this latitude was nearly $6^h\ 23^m$. On the 6th instant it was nearly $5^h\ 25^m$. This difference of time in rising compensates the difference of time in the greatest phase of the two eclipses, therefore there is no great variation of the altitudes.

At the nearest approach of the greatest phase last time, the column of the solar radiation thermometer decreased 1·1, but this

* Pages 72 and 73, Vol. xxxii. No. 3. *Royal Astronomical Society's Monthly Notices.*

time only ·5, notwithstanding the magnitude of the eclipse was so great as ·927. In the present case the humidity was apparently great. I wonder what else could cause the less decrease of the column, except the absence of clear sky, and the presence of clouds between the Sun's rays and the Earth, which may probably impede terrestrial radiation also.

Meteorological Observations taken during the time of the Eclipse, 6th June, 1872.

Hours.	Barometer reduced to 32°.	Dry bulb.	Wet bulb.	Humidity.	Solar radiation thermometer.	Kind of clouds.	Clear sky.	Direction of wind.
h m	inches	°	°	cents	°		cents	points
6 3	29·687	88	82	76	51	cu	60	S.
6 13	29·689	88	82	76	51·4	cu	20	S. by W.
6 23	29·695	88·4	82	76	52·4	cu	20	W.S.W.
6 33	29·695	88·2	82	76	51·6	cu	30	S.S.W.
6 44	29·697	88	81·5	74	51·2	cu	20	S.
6 43	29·699	88	81·5	74	50·5	cu	20	S.
7 3	29·699	88	81·5	74	50	N.	,,	S.
7 13	29·701	88	81·5	72	49·5	N.	,,	S.W.
7 23	29·705	87·8	81·4	76	50·5	N.	,,	S.
7 33	29·709	87·7	81·3	76	52·3	cu	20	S.
7 43	29·709	87·7	81	72	54	cu	20	S.W. by W
7 53	29·709	88	81·4	72	55·4	cu	20	S.W.
8 3	29·711	88	81·7	76	57·3	cu	30	S.W. by W.
8 13	29·715	88	81·8	76	59·3	cu	30	S.W. by W.
8 23	29·721	88	81·8	76	63	cu	50	S.W.
8 30	29·720	88·6	81·7	73	76	cu	50	W. by S.

Observations made during the Eclipse of June 6, 1872. By N. R. Pogson, Esq., Government Astronomer at Madras.

(*From a Letter to the Astronomer Royal.*)

I take the earliest opportunity of informing you of our results on the occasion of the annular eclipse of the Sun on the 6th.

Availing myself of your kind encouragement to make use of the Browning reflector, I got the Government to sanction the immediate erection of a sliding flat-roofed room, a dark room, and a printing room, over the transit-circle room, and got all completed just, and only just, in time. My son, assisted by Mr. F. Doderet, got nine photographs altogether—four during the important time. I enclose copies of six: No. 1, partial, before centre; 2, just as the ring was forming, when the coronal light,

shining behind the Moon's disk, made the ring appear faintly completed before it really was so. This was clearly seen also by Ragoonatha Charry with the Lerebours equatoreal, and even by myself and my son with the finders of the Simms' equatoreal and of the Browning reflector. The photograph shows it, but the negative better still. No. 3 shows the complete ring at central time, 4 near its breaking, 5 at the moment, and 6 a partial phase soon after. Nos. 4 and 5 got taken on the same plate by a mistake, but, by placing one in the centre and painting the other out, there is no confusion. No prominences were seen, either by the eye or in the photographs, though the spectroscope indicated plenty about, especially near the spot shown in No. 4.

I resolved to attempt nothing but the spectroscope, and managed far better than at Avenashy in consequence. I wanted to have had the time of first contact from the spectroscope, by the extinction of the usual bright C, D, and F lines of the chromosphere. I can rarely find any part of the Sun's limb on a fine day at which these are not visible, and I thought that the contact of the limbs should extinguish these; but the Sun was too low to be well got at, and, unless the slit were very accurately over the point of first contact, and reduced in length as well as in breadth till it had nearly become a square, I do not believe in a result. At the first internal contact (just after a peep in the finder had shown me the Moon's limb lighted up by the corona) I saw all the dark lines reversed and bright, but for less than two seconds; the thinnest thread of sunlight restored them instantly.

I could see no trace of lines on the Moon's dark centre, but probably this was owing to the flash of ring-light having dazzled my eyes too much, or else atmospheric dark lines would, I suppose, have been there. The sight of beauty, above all, was, however, the reversion of the lines at the breaking up of the limb. *All* lighted up, reminding me of the platinum wire in the Oxford heliometer, seen through the object-glass some distance off, when Mr. Johnson was putting on the battery to read the scale. The duration astonished me—five to seven seconds; and they faded out gradually, not momentarily. I had even time to turn the micrometer cross-wires upon the most brilliant line of all, which I took for C at the moment, but which the reading clearly showed was B when all was over.

I think these results will interest you; and besides ordinary records, which were duly cared for and will all be ready in due course, I know of nothing else to be expected from an annular eclipse.

The negatives shall be forwarded with these of the Avenashy expedition as soon as the Report is ready; but these eclipses are fearful interruptions, and Avenashy completely upset all my arrangements for last year, and this one, so far, as well.

Madras Observatory, June 11th, 1872.

On Future Solar Eclipses. By the Rev. S. S. Johnson.

The following list of eclipses was undertaken, in the first instance, for my own information and curiosity. Any investigation with regard to phenomena a vast number of years hence can only be a subject of mere curiosity; nevertheless, it may not be regarded as wholly uninteresting. If it be said to be needless to have continued the following examination to the end of the twenty-second century, I can only refer to the computations of the transits of *Venus*, of which Delambre has given a list for a period of two thousand years. I may therefore be excused for calling attention to the eclipses for only *three* centuries to come. The chief reason that induced me to do so was to endeavour to discover, if possible, when the next eclipse of the Sun will take place that will be total at London. Although it is now known what will be the date of this phenomenon for England, I believe no attempt has been made to find out when such will be the case at London. The eclipses of 1912 (in one work) and 1916 (in several works) have been referred to as satisfying the conditions. This is now known to be an error. When it is merely desired to *predict* an eclipse, the long and intricate calculations of the present day are not needed: it is enough to know the time approximately, and not to the nearest second. If expeditious tables could be found, giving the time to a few minutes, they would suffice. Such tables, I believe, I have discovered in those given in the eighth edition of the *Encyclopædia Britannica*. First of all, I tried a great number of *known* solar eclipses by them. In only one instance, that of May 26, 1873, have I found them more than nine minutes out in the time of the greatest obscuration, generally very near indeed, and the size of the eclipse has always been correct in every case I have tried. Amongst others, I tried the eclipse of 1140 by them, which was always thought to be total at London, according to Dr. Halley, until Mr. Hind published his computations in the summer of 1871. After computing by these tables in the *Encyclopædia*, I found, upon making a projection, that a crescent was uncovered at the south side of the Sun, showing the shadow must have gone to the north of London. This agreed with Mr. Hind's result. I therefore concluded that these tables would suffice for the purpose. I am not aware of any list of future eclipses extending over many years. In the *Mémoires* of the French Academy (1768) there is a paper by M. du Vaucel on all the solar eclipses visible at Paris from 1767 to 1900. This catalogue was computed from the tables of Mayer for the meridian of Paris, in order to gratify the French king, who was anxious to know whether a total or annular eclipse would happen soon. I have heard of Hallasckha's list of future eclipses, but do not know how far it extends. Beyond the limits of the present century but little mention has been made of eclipses. I do not, of course, forget Mr. Hind's communication to

the *Times*, last year, concerning the tracks of certain of them in the next century, and also of many in the remainder of the present. One he has omitted, that of May 28, 1900, which I believe is total in the south of Spain, and consequently the next occurring within easy distance from this country.* I have seen lists of eclipses to the end of the present century in two or three astronomical works. There was, therefore, no need to give them. I have, accordingly, commenced with the next. As to the main object I had in view—to discover, if possible, when the next total eclipse for London takes place—the eclipses of 2090 and 2151 seem both likely to satisfy the conditions, but in the former case totality takes place by these tables about $5^h\,34^m$, or only about a quarter of an hour before sunset. Consequently I continued the computations into the following century. In the eclipse of June 14th, 2151, I believe London will be included within the belt of totality, to the southward of the central line.

Eclipses of the Twentieth Century.

The next very large solar eclipse takes place forty years hence, on April 17, 1912. This is a return of that which excited so much interest in 1858. The Moon's semidiameter does not quite equal the Sun's. At London only a narrow crescent on the upper part of the Sun's disk is left uncovered, soon after mid-day. There is another in 1961, but the greatest obscuration seems to take place close upon sunrise. That of 1999 Mr. Hind has shown to be total in the west of England. An eclipse on the morning of April 8th, 1921, is evidently annular in the northern parts of the kingdom, and consequently the next central eclipse we shall have in the British Isles. I have not seen this eclipse anywhere referred to. It will not be so large at London as that of 1912. The only other eclipse of any size occurs on June 30, 1954, but then less than nine-tenths of the Sun's upper limb are obscured at the metropolis about half-past twelve.*

I have expressed the size of each eclipse by the old method of digits, or twelfth parts of the Sun's surface. M. signifies morning, A. afternoon.

Date.			Greatest Obscuration. h m	Digits Eclipsed.
1905, August	30	..	about 1 4 A.	10
1908, June	28	..	„ 5 40 A.	2
1912, April	17	..	„ 0 25 A.	11
1914, August	21	..	„ 11 57 M.	8
1916, February	3	..	sets eclipsed.	
1919, November	22	..	„	
1920, „	10	..	„	

* That of 1887 can hardly be regarded, as occurring so near upon the time of sunrise.

There is a large eclipse in 1927, but I doubt its being total or annular anywhere.

Date.			Greatest Obscuration. h m	Digits Eclipsed.
1921, April	8	..	about 8 53 M.	10
1922, March	28	..	" 2 8 A.	2
1925, January	24	..	" 3 50 A.	7
1927, June	29	..	" 5 12 M.	11
1928, November	12	..	" 8 28 M.	2
1929, "	1	..	" 11 37 M.	1
1936, June	19	..	" 4 15 M.	6
1939, April	19	..	" 6 19 A.	4
1942, September	10	..	" 4 20 A.	4
1945, July	9	..	" 1 57 A.	7
1949, April	28	..	" 7 29 M.	4
1952, February	24	..	" 8 55 M.	1
1954, June	30	..	" 0 28 A.	10
1959, October	2	..	" 0 21 M.	4
1961, February	15	..	" 7 28 M.	11
1966, May	20	..	" 9 28 M.	6
1968, September	22	..	" 10 15 M.	4
1971, February	25	...	" 9 31 M.	7
1972, July	10	..	" 8 3 A.	6
1973, December	24	..	sets before the middle.	
1975, May	11	..	about 6 29 M.	6
1976, April	29	..	" 10 17 M.	4
1982, December	15	..	" 8 16 M.	5
1984, May	30	..	" 6 13 A.	5
1994, "	10	..	" 6 45 A.	6
1996, October	12	..	" 2 27 A.	7
1999, August	11	..	" 10 8 M.	11

Eclipses of the Twenty-first Century.

Those of 2026 and 2081 appear to be total in France. That of 2093 is annular, and probably so at London ; but the central line appears to run a little north of it. That of 2090 I have already alluded to. Column (1) shows date, (2) approximate hour of greatest obscuration, (3) digits, or twelfth part of the Sun's surface eclipsed.

(1)			(2) hour	(3)
2003, May	31	..	rises eclipsed.	
2005, October	3	..	$9\frac{1}{4}$ M.	7
2006, March	29	..	$10\frac{1}{4}$ M.	3
2008, August	1	..	9 M.	2
2011, January	4	..	sunrise	8
2015, March	20	..	$9\frac{1}{2}$ M.	10
2017, August	21	..	7 A.	2

(1)			(2) hour	(3)
2021, June	10	..	10½ M.	3
2025, March	29	..	11½ M.	4
2026, August	12	..	6 A.	11
2027, "	2	..	9 M.	5
2028, January	25	..	4¼ A.	7
2030, June	1	..	5¼ M.	7
2036, August	21	..	6 A.	8
2037, January	16	..	8¼ M.	7
2038, July	2	..	2 A.	1
2039, June	21	..	6½ A.	9
2048 "	11	..	1½ A.	9
2050, November	14	..	2 A.	9
2053, September	12	..	8¼ M.	7
2059, November	5	..	8 M.	9
2060, April	30	..	10¼ M.	2
2066, June	22	..	8¼ M.	9
2069, April	21	..	10 M.	4
2075, July	13	..	4¼ M.	10
2076, November	26	..	11 M.	5
2079, May	1	..	11 M.	5
2080, September	13	..	4¼ A.	9
2081, "	3	..	7½ M.	11
2082, February	27	..	4 A.	6
2088, April	21	..	10½ M.	6
2090, September	23	..	5½ A.	12
2091, February	18	..	10 M.	6
2092, "	6	..	4¼ A.	7
2093, July	23	..	0¼ A.	11

Eclipses of the Twenty-second Century.

For this century I find four very large eclipses. Those of 2135 and 2200 seem total in England north of London, but in the last instance the Moon's semidiameter does not much exceed the Sun's. The totality of that of 2142 will, I believe, go southward of this country. That of 2151, June 14, attains the greatest obscuration or totality at London, by these tables, about $6^h\ 27^m$. The following are the approximate times of greatest obscuration :—

				hour
2135, October	7	..	..	7¼ M.
2142, May	24	..	..	8¼ M.
2151, June	14	..	..	6½ A.
2200, April	14	..	..	5¼ A.

Upton Helions Rectory, Crediton.

*On the Orbit of the Binary Star ξ Ursæ Majoris.**

By Dr. Robert S. Ball.

The object of this paper is to compare some elements which have been computed, with recent observations; to propose a new set of approximate elements; and to give an approximate ephemeris.

The last periastre occurred in 1816·4. Prior to this date the only observations are those of Herschel I. in 1781, 1802, 1804. In the interval of fifteen years from 1804 to 1819 the position-angle swept through 168°, yet no observation has been recorded during this critical period.† The star will reach periastre again in 1876 3, consequently recent observations have to a great extent replaced those wanting between 1804 and 1819.

The four sets of elements here examined are taken from Herschel's *Outlines*, 4th edition, p. 573. Savary's value of the inclination is 59° 40′. By a misprint it is given 50° 40′ in the *Outlines*.

Table I. *ξ Ursæ Majoris.*

Eccentricity. e	Position of Node. Ω	Inclination. γ	Angle between Major Axis and Nodes. λ	Period in Years. P	Perihelion Passage A.D. t	Authority.
	° ′	° ′	° ′			
0·41640	95 22	59 40	131 38	58·262	1817·25	Savary.
0·37770	97 47	56 6	134 22	60·720	1816·73	Herschel II.
0·41350	98 52	54 56	130 48	61·464	1816·44	Mädler.
0·43148	95 50	52 49	128 57	61·576	1816·86	Villarceaux.

In the comparison of these elements with observation, twenty-two observations have been selected.‡ These observations are distributed as well as possible over the ninety years during which the star has been observed; and whenever a choice has been possible, results of the greatest weight have been taken. The result of the comparison is shown in Table II. (See next page.)

It will be noticed that Savary's elements represent the observations up to No. 6 very well. After No. 6 the calculated position falls behind the observed; the difference reaches a maximum in No. 13, where it amounts to −9°·44. Savary's elements, though the earliest in date, represent the orbit fairly.

The elements given by Herschel II. give for No. 22 a calculated position 17°·43 in excess of the observed position: with this exception, the greatest difference is −6°·06 in No. 13. With reference to this determination, Sir John Herschel remarks, "The case is one highly unfavourable for the application of my method, and, moreover, the resulting elements are those of a first approximation only."

* Read before the Royal Irish Academy.

† At least no such observation is recorded in Mr. Dawes' invaluable list, *M. R. A. S.* vol. xxxv. p. 353.

‡ Many of these observations have been taken from Dawes' list. Others have been supplied by the kindness of Dr. Brünnow.

Table II. ξ *Ursæ Majoris.*

No.	Observer.	Epoch.	Observed Position.	Savary. Calculated Position.	Savary. Diff. from Observed Position.	Herschel II. Calculated Position.	Herschel II. Diff. from Observed Position.	Mädler. Calculated Position.	Mädler. Diff. from Observed Position.	Villarceaux. Calculated Position.	Villarceaux. Diff. from Observed Position.
			°	°	°	°	°	°	°	°	°
1	Herschel I. ..	1781.97	143.78	143.89	+0.11	140.08	− 3.70	142.85	− 0.93	144.12	+ 0.34
2	„ ..	1802.09	97.52	97.40	−0.12	93.92	− 3.60	97.72	+ 0.20	97.73	+ 0.21
3	„ ..	1804.08	92.63	93.07	−0.34	88.52	− 4.11	92.45	− 0.18	92.55	− 0.08
4	Struve ..	1819.10	284.55	284.97	+0.42	285.13	+ 0.58	284.54	− 0.10	285.60	+ 0.05
5	Herschel II. and South	1823.29	258.45	257.45	−1.00	258.37	− 0.08	256.60	− 1.85	254.78	− 3.67
6	South ..	1825.22	244.53	245.50	−0.97	246.45	+ 1.92	244.47	− 0.06	242.38	− 2.15
7	Struve ..	1827.27	228.27	231.50	+3.23	232.68	+ 4.41	230.97	+ 2.70	229.03	− 0.76
8	Dawes ..	1832.27	196.72	191.69	−5.03	195.40	− 1.32	196.39	− 0.33	196.43	− 0.29
9	„ ..	1840.29	150.85	143.65	−7.20	148.92	− 1.93	153.80	+ 2.95	155.48	+ 4.63
10	„ ..	1842.27	144.76	136.50	−8.26	141.48	− 2.28	146.60	+ 1.84	148.33	+ 3.57
11	„ ..	1843.28	142.17	133.29	−8.88	138.15	− 4.02	143.35	+ 1.18	144.98	+ 2.81
12	„ ..	1849.30	126.58	118.44	−8.14	122.27	− 4.31	127.47	+ 0.89	128.58	+ 2.00
13	Miller ..	1852.13	122.28	112.84	−9.44(!)	116.22	− 6.06	121.37	− 0.91	122.22	− 0.06
14	Jacob ..	1853.20	119.47	110.85	−8.62	114.03	− 5.44	119.17	− 0.31	119.93	+ 0.46
15	Dawes ..	1854.36	115.87	108.72	−7.15	111.73	− 4.14	116.85	+ 0.98	117.55	+ 1.68
16	Dembowski ..	1858.20	108.09	101.67	−6.42	104.12	− 3.97	109.29	+ 1.20	109.70	+ 1.61
17	„ ..	1863.23	96.66	90.92	−5.74	92.85	− 3.81	98.54	+ 1.88	98.48	+ 1.82
18	..	1865.12	91.42	85.59	−5.83	87.55	− 3.87	93.67	+ 2.25	94.00	+ 2.58
19	Dembowski ..	1866.30	86.76	81.50	−5.26	83.65	− 3.11	90.20	+ 3.46	90.68	+ 3.92
20	„ ..	1867.31	82.22	77.24	−4.98	79.78	− 2.44	86.82	+ 4.60	87.52	+ 5.30
21	„ ..	1868.30	77.50	72.02	−5.48	75.27	− 2.23	83.02	+ 5.52	84.02	+ 6.52
22	Brünnow ..	1872.28	24.19	20.57	−3.62	41.62	+17.43(!)	57.19	+33.00(!)	62.10	+37.91(!)

Mädler's elements represent the first eighteen observations with surprising fidelity, and there is no very serious discrepancy until No. 22, when the difference is [illegible].

Villarceau's elements give for No. 22 a discrepancy of [illegible].

There cannot be a doubt that, had the four eminent astronomers who have been mentioned had the advantage of several observations in the neighbourhood of periastre, their orbits would have been verified by the recent observations. Three of the orbits, Savary's being the exception, appear to have too large a value for the periodic time. This is the principal source of the discrepancies. In the absence of Mr. Brünnow's observation No. 22, and some others preceding it, Mädler's elements would have fair claims, but it will be evident that some change is rendered necessary by the recent observations.

By the aid of Sir John Herschel's beautiful graphic process, *M. R. A. S.* (vol. v. p. 209), the following approximate elements have been determined:—

$$\Omega = 103^{\circ}{\cdot}6$$
$$\gamma = 53{\cdot}1$$
$$\lambda = 135{\cdot}3$$
$$P = 59{\cdot}88$$
$$\tau = 1816{\cdot}405$$

These elements have been compared with the twenty-two observations already mentioned, and also with two more, Nos. 22 and 23 of the following Table:—

Table III. ξ *Ursæ Majoris.*

Comparison of the new Approximate Elements,

No.	Authority.		Epoch.	θ_o the Observed Angle of Position.	θ_c the Computed Angle of Position.	Difference $\theta_o - \theta_c$
				°	°	°
1	Herschel I.	..	1781·97	143·78	147·37	+3·59
2	,,	..	1802·09	97·52	98·53	+1·01
3	,,	..	1804·08	92·63	92·55	−0·08
4	Struve	..	1819·10	284·55	287·93	+3·38
5	Herschel II. and South		1823·29	258·45	259·20	+0·75
6	South	..	1825·22	244·53	246·20	+1·67
7	Struve	..	1827·27	228·27	231·63	+3·36
8	Dawes	..	1832·27	196·72	195·13	−1·59
9	,,	..	1840·29	150·85	152·95	+1·10
10	,,	..	1842·27	144·76	145·98	+1·22
11	,,	..	1843·28	142·17	142·85	+0·68
12	,,	..	1849·30	126·58	127·10	+0·52
13	Miller	..	1852·13	122·28	120·87	−1·41

No.	Authority.		Epoch.	θ_c the Observed Angle of Position.	θ_c the Computed Angle of Position.	Difference. $\theta_c - \theta_c$
				°	°	°
14	Jacob	..	1853·20	119·47	118·58	−0·89
15	Dawes	..	1854·36	115·87	116·12	+0·25
16	Dembowski	..	1858·20	108·09	107·80	−0·29
17	,,	..	1863·23	96·66	94·87	−1·79
18		..	1865·12	91·42	88·43	−2·99
19	Dembowski	..	1866·30	86·76	83·52	−3·24
20	,,	..	1867·31	82·22	78·55	−3·67
21	,,	..	1868·30	77·50	72·58	−4·92(!)
22	,,	..	1870·24	57·74	56·10	−1·64
23	,,	..	1871·22	47·70	43·80	−3·90
24	Brünnow	..	1872·28	24·19	26·47	+2·28

The greatest discrepancy is in No. 21, where the difference amounts to $-4^\circ{\cdot}92$.

The angle of position is at present changing at the rate of nearly 20° per annum, consequently observations during the next few years will be of the greatest service in sidereal dynamics.

From the approximate elements which have been given the following approximate ephemeris has been computed A comparison of the positions given in this ephemeris with observations may perhaps provide materials for the final correction of the elements. The angle of position is computed at intervals of three months from 1872·50 up to 1878·75. The star will pass periastre in 1876·28.

Table IV. Approximate Ephemeris of the Angle of Position of *ξ Ursæ Majoris.*

No.	Epoch.	Angle of Position.	No.	Epoch.	Angle of Position.
		°			°
1	1872·50	22·4	14	1875·75	321·4
2	·75	17·5	15	1876·00	317·9
3	1873·00	12·5	16	·25	314·7
4	·25	7·3	17	·50	311·6
5	·50	2·2	18	·75	308·6
6	·75	357·2	19	1877·00	305·8
7	1874·00	352·1	20	·25	303·2
8	·25	347·0	21	·50	300·7
9	·50	342·2	22	·75	298·4
10	·75	337·6	23	1878·00	296·1
11	1875·00	333·2	24	·25	293·9
12	·25	329·0	25	·50	291·8
13	·50	325·1	26	·75	289·7

Essay on the Arc of the Meridian measured in Lapland. By I. Todhunter, M.A., F.R.S. *Transactions of the Cambridge Philosophical Society*, vol. xii. part I.

In this essay, Mr. Todhunter gives a series of valuable and interesting critical remarks on the accounts which have been given of the two measurements of the celebrated Lapland arc. He points out many (and some most remarkable) errors in these accounts, and applies the appropriate corrections.

It would be altogether impossible to give an abstract of the essay, which is already singularly condensed. Every line should be carefully studied by those who desire to know the real history of the important operations in question.

The following reflections are instructive and suggestive:—

"It would be a curious subject of speculation whether the theoretical opinions of persons engaged in geodetical surveys could have exercised any influence on their observations; I mean of course unconsciously, for it would be wrong to suspect any deliberate unfairness in any of the operations which I have examined. From a passage in the article *Figure de la Terre* by D'Alembert, in the original *Encyclopédie* it would appear that the school of Cassini originally believed that in consequence of the oblate form of the Earth, the length of a degree of the meridian *would decrease* from the equator to the pole. It seems strange, perhaps, now to suppose that such an error could be seriously maintained; but there can be no doubt of it: for example, the error was vehemently maintained by Keill, a man of some reputation, who was ultimately a Savilian professor at Oxford. See Keill's *Examination of Dr. Burnet's Theory of the Earth*, p. 140. It is certainly a remarkable coincidence that the school of Cassini, starting with the erroneous theoretical notion that the degrees of the meridian *ought* to decrease from the equator to the pole, arrived at the same result by observation and measurement. There can, I think, be no doubt that at least Maupertuis and Clairaut, who were the most eminent of the French party, held the correct Newtonian theory as to the figure of the Earth; and their result was rather too decided in its confirmation of this theory. Now the geodetical angles could scarcely be influenced by the theoretical opinions of the observers; because it would not be obvious in what way the result would be affected by an error in an angle. But in measuring the base it would of course be obvious that the larger was the value obtained, the stronger was the evidence for an oblate form. Similarly in estimating the amplitude, the smaller the value obtained the stronger was the evidence for the oblate form. In these two parts of the survey then it would be necessary to be on the watch lest the conviction of what the result ought to be should influence the impression of what the observation really gives. It is curious that Maupertuis and his party seem to have thought at first that

their success was too decided and therefore their amplitude too small; and that on their second determination they should have made it between 3″ and 4″ larger than at first. Svanberg, I consider, was sent to Lapland with a strong expectation that he would obtain a less value of a degree of the meridian than that of Maupertuis. See Melanderhjelm's preface to Svanberg's book."

Mr. Todhunter makes the following remarks on a question of mathematical history:—

"Svanberg alludes to the researches of Huygens on centrifugal force; and then he says that if we suppose with Huygens that the force of gravity resides in the centre of the Earth we obtain for the ellipticity $\frac{1}{578 \cdot 73}$. Then he proceeds with the strange statement that these discoveries were the precursors of the hypothesis which Newton afterwards proposed of universal gravitation. Thus Svanberg ascribes to Huygens and not to Newton the priority in investigations of the figure of the Earth. Now the first edition of the *Principia* was published in 1686. Huygens published in 1690 his *Discours de la cause de la Pesanteur*. Although it appears from the preface that some part of this work had been long before communicated to the Academy at Paris, yet Huygens excepts the part which relates to the motion of pendulums as affected by the figure of the Earth: and all that relates to the calculation of the figure of the Earth he expressly says was added after the publication of Newton's *Principia*. Perhaps Svanberg was misled by a hasty glance at Bailly's *Histoire de l'Astronomie Moderne*, vol. iii. p. 9. Bailly accidentally notices Huygens before Newton. Again Svanberg says, 'Newton ne tarda pas à remarquer, que l'hypothèse de Huygens d'une force unique tendante au centre de la terre, étoit équivalente à celle d'y supposer une densité infinie.' I do not know what this means. Huygens really used the hypothesis of a force constant in magnitude, and directed always to the centre of the Earth; it is of course only as to the latter part of the hypothesis that we can assert the equivalence of an infinite density at the centre. I do not think that Newton drew attention to this equivalence: Clairaut did in the *Philosophical Transactions*, vol. xl. pp. 277–306. Clairaut, I think, was the first who showed that Huygens' value of the ellipticity follows approximately from the latter part of Huygens' hypothesis, namely, that the force always passes through the centre, without assuming any particular law of intensity of the force at different distances. See Clairaut's *Figure de la Terre*, p. 141. See also Plana, *Astronomische Nachrichten*, No. 839."

The Orbit of Castor. By J. M. Wilson, Esq.

In the last number of the *Monthly Notices* I stated that the result of an examination of the orbit of *Castor* by graphical

methods was that it was hyperbolic. These methods are rough, and in this case present peculiar difficulties. Since writing that note, I have attacked the question analytically, following the method of Sir John Herschel, in *Mem. Ast. Soc.* Vol. xviii., and have the pleasure to acknowledge much help in the arithmetical part of the work from my friend Mr. Henry Stevenson, of Bedford, who undertook the laborious task of solving the equations.

The method is as follows: The polar co-ordinates, r, θ, of the determined points are converted into rectangular co-ordinates, x, y. An equation is then assumed to a conic section, which passes through these points,

$$0 = 1 + \alpha x + \beta y + \gamma x^2 + \delta xy + \epsilon y^2,$$

and by substituting the values of x, y for the points determined, as many equations are found for α, β, γ, δ and ϵ as there are points. These are combined by the method of least squares, so as to give the most probable result. I omitted the point corresponding to 1740·46 as resting on very insufficient evidence; and I am inclined to believe that a still better result would be obtained if the next two points were also omitted, or had much less weight assigned them.

By a singular coincidence, in the solution of the equation, the coefficients of x^2 and y^2 come out so small as not to affect the first seven places of decimals, and hence the curve is seen at once to be approximately a rectangular hyperbola, whose asymptotes are parallel to the axes, and whose eccentricity is 1·414.

The resulting equation is

$$0 = 1 + \cdot 008876x + \cdot 008098y + \cdot 0000433xy,$$

A being the origin and the positive parts of the axes of x and y, being in the directions 90° and 180° measured in the usual manner from the north.

The eccentricity of the real orbit may hence be approximately computed at 1·625.

The divergence of this result from that obtained by graphical construction is at first sight very great, for by that the eccentricity of the apparent orbit was more than 2. But the graphical solution is not to be trusted, and in this case it is easy to make errors that prejudicially affect the eccentricity. One of my pupils—Mr. D. C. Baynes—made the curve nearly parabolic on one occasion, and on another he made the eccentricity 2·01. An error of a tenth or twentieth of an inch in measurement, or a corresponding error in drawing the hyperbola, would vitiate the result completely. To determine the centre and axes of an incorrectly drawn hyperbolic arc in the neighbourhood of the vertex is a very unsatisfactory problem. The analytically obtained result has more weight: that is, it has the weight, to the degree of approximation to which it has been carried, of that combination of the original observations which is given by the interpolating curve, smoothed by the method of differences to the third order, with no errors of drawing superadded.

I have here a chart* showing all the observations that have been sent me, from which it can be judged how far the interpolating curve adopted is a fair representation of them; and a roughly drawn chart marking the positions of the stars at different dates, and the supposed hyperbolic arc on which they lie. The deviation of the curve from the early positions is great, but they rest on very few observations, as is seen from the other chart, and several of the early observations, as of the late ones, are many degrees in error.

It occurred to Mr. Seabroke and myself that it would be worth while to examine the spectra of A and B, to see whether the indication derived from the orbit, that they belonged to different systems, was confirmed; but the long delay of an optician in preparing part of the necessary apparatus has frustrated us for the present.

There appears to be something peculiar about the motion of this pair. Every successive determination of the eccentricity of the relative orbit has increased that eccentricity, as shown by Mr. Proctor in his note on my former paper. It may be thought worth while, by some one with more skill or more leisure than I possess, to subject the orbit to a searching examination, and this is my object in bringing the matter in a confessedly unfinished state before the Society.

My own conclusion is, that it is now probable that the orbit is really hyperbolic.† If so, *Castor* must be looked on, not as a binary system, but as two stars, whose relative proper motions have been in nearly opposite directions, and whose near approach has been witnessed once, and will never be witnessed again.

Temple Observatory,
Rugby, June 14, 1872.

On the Desirability of Watching for the November Meteors in the present year. By Rich. A. Proctor, B.A. (Cambridge.)

On the night of November 13–14, 1871, no members of the November meteor system were observed in England; and the conclusion was arrived at that the richer portion of the system has passed so completely beyond the Earth's orbit that no displays may be expected until the close of the century. Mr. Glaisher, in fact, promised his meteor-observing assistants at Greenwich that they should no longer be kept on the watch after midnight on November 13-14. There are reasons for believing, however, that the conclusion on which this promise was based is incorrect. What appears to have happened is simply this, that a more expanded portion of the meteor stream is now traversing the Earth's orbit, and that accordingly the passage of the Earth through the system

* The publication of this chart is unavoidably delayed.

† My friend and former pupil, Mr. A. M. Worthington, of Trinity College, Oxford, has worked the whole question over again from the original data, and makes the orbit a hyperbola of small eccentricity, *i. e.* nearly parabolic.

lasts longer than in former years, while, owing to the irregular constitution of the system, several successive hours may pass without any meteors being seen.

For, although the night of November 13-14, was not attended (in England and France at least) with any display of the Leonides, yet on the night of November 14-15, many meteors were seen at certain stations where the weather was favourable. Thus, at Alexandria, Prof. Parnisetti, and five assistants, obtained the following results:—

Between 10	and	11 P.M.,	Nov. 14,	3	meteors were counted.
11	"	12	"	6	"
12	"	1 A.M.,	" 15,	19	"
1	"	2	"	23	"
2	"	3	"	84	"
3	"	4	"	100	"
4	"	5	"	95	"
				300	

As the sky was not at any time quite clear, this result shows that between 2 and 5 a veritable meteoric display was in progress.

At Volpeglino, near Tortona, Signor Maggi observed 52 meteors (most of them Leonides) between 3.15 and 6.15 A.M. The sky was much clouded, however.

At Genoa, Professor Garibaldi counted more than 100 meteors between 8.15 P.M. Nov. 14, and 1.30 A.M. Nov. 15.

It is even more deserving of notice, however, that at Moncalieri on the night of November 16-17, no less than 128 meteors were counted, of which the majority were Leonides. The significance of this observation will be understood when it is remembered that the Earth had traversed nearly 3 millions of miles between the epochs of the observations made at Alexandria and Moncalieri.

It may be added that in 1870 Capt. Tupman had noticed that Leonides began to fall in considerable numbers two or three days before November 13-14.

It would appear that instead of discontinuing the observations altogether, what is now wanted is that a watch should be maintained during the hours between 10 P.M. and 6 A.M. (say) from November 10 to November 17. I fear this suggestion is not likely to be unanimously welcomed.

Prof. Denza remarks of the passage last year, that 'the meteor-cloud has appeared not only to be less dense, but displaced from its usual position, very turbid (*troublé*) and irregular; for the portion which the Earth traverses at present is but a remnant, and, as it were, a tenuous tail behind the more densely crowded central group.' 'It is to be inferred, also,' he adds, 'that the flight of meteors is extending itself gradually along its orbit,—a process which will in the long run cause the whole orbit to be occupied by cosmical matter, after which there will be displays of the meteors not merely at intervals of 33¼ years, but year after

year, and with about the same degree of richness, precisely as in the case of the August meteors, or Perseides, whence we can understand the great interest attaching to the study of these objects during the next few years, for the purpose of tracing the law according to which the progressive diminution of these displays proceeds, especially if we remember (as Prof. Schiaparelli, of Milan, has pointed out to me), that a fine shower was observed in 1818, that is to say, nearly midway between the great displays of 1799 and 1833.'

A General Comparative Table of Radiant-Positions and Duration of Meteor-Showers. By R. P. Greg, Esq., F.R.A.S.

The accompanying Table of Radiant-Positions and Epochs of Meteor-Showers is intended to show at a glance the comparative results of all the meteoric showers whose centres of radiation, in right ascension and north declination, and whose epochs have hitherto been approximately or definitely ascertained. The recent increased interest attached to meteoric science since Professor Schiaparelli's discovery of the intimate connexion between the orbits of comets and meteor-rings renders it most desirable that the scattered and laborious researches of various observers and collators should be brought together and synoptically arranged. This I have endeavoured to do in as brief and simple a manner as possible. Having due regard to the nature of the case, there results a greater amount of coincidence and resemblance than might have been expected from the reductions of nearly 6000 observations, independently of Leonides and Perseides, made by different observers in different countries, recorded during the last twenty years. Indeed the results are frequently remarkably coincident, when we consider how difficult it is to arrive, even in the case of the more frequently observed and better known Leonides and Perseides, at a precise radiant-point. It is even less simple to ascertain with anything like precision the extent and configuration of the radiant area when, as is frequently the case, it more or less diffusely occupies a region amongst the stars of some 10° or 20° in diameter. In some cases it is highly probable that the area is simply "diffuse;" in others it probably includes a series of minor or sub-radiants (the existence and origin of which have been ingeniously, and perhaps correctly, explained by Signor Schiaparelli*), and of whose existence I had myself expressed a suspicion, some years ago, in the Report of the British Association for 1868–9. The probability of the existence, also, of meteoric showers having a duration of some six weeks appears to be still further confirmed by the comparative table or catalogue now presented. (See *Proceedings of the British Meteorological Society* for January 1865.) Professor Heis (see the *Astronomische Nachrichten*, April 1867) was the first systematically to attempt to group and arrange centres of

* See the Report of the British Association Committee on Luminous Meteors, 1871.

radiation for meteor-showers, as deduced from a series of many hundreds of observations recorded and mapped by himself and pupils at Münster, in Westphalia; but he did not attempt to fix the exact duration, or radiant-position, of individual meteor-showers, or the relative importance of the different meteoric systems. Messrs. Greg and Herschel attempted to do this more systematically, both from special observation and by a reduction of some 2000 meteors recorded in the annual Meteor Reports and Catalogues of the British Association (see the vols. of the British Association Reports for 1864, and 1868–70). The results were also laid down in a series of 22 folio plates upon an atlas of the stars gnomonically projected. In addition, a number of new radiants were also determined by myself, deduced from some 700 Italian observations observed and described by Signors Zezioli, Denza, and Serpieri, during the years 1868–1870; and many of the English ones were found to be fairly well confirmed by the Italian observations. The catalogue of meteor-radiants lately published by Schiaparelli, chiefly and specially deduced from some 900 (out of nearly 7000) observations made in the course of three years, 1867–1869, by the late indefatigable observer Signor Zezioli, of Bergamo, was drawn up with the more special view of tracing out the specific centres of radiation of various separate or identical meteor-showers, and their possible daily alterations of position. The result seems frequently to confirm a number of the radiants of Professor Heis and of Messrs. Greg and Herschel, and to indicate more clearly the probable occurrence of subordinate or multiple radiant-points belonging to one and the same meteor-system. (See the British Association Reports, 1871, p. 44, *et seq.*) It may also be here noted that the Italian observations, generally more consecutively and regularly conducted, include, necessarily, a far larger proportion of small meteors, of 4th and 5th magnitudes, than could be the case with the English and German ones; and also that, though including a greater number of probable meteor-showers of minor and secondary importance, these nevertheless possess features of much scientific interest. Probably the catalogue of radiants given by Messrs. Greg and Herschel show best the general positions, epochs, and durations of the more prominent showers; but much has yet to be done, doubtless, in more precisely and accurately determining those features. The very considerable number of centres of radiation, combined with the frequent, and evident tendency to width and diffuseness in some of the radiant areas themselves, combine to make the reductions and grouping of the paths, or meteor-tracks, for any single radiant, on celestial maps especially constructed for the purpose, often a matter of considerable complexity and difficulty. Mr. Greg thought at first that there was a not unfrequent tendency, in long-enduring showers, to give a radiant advancing with the time among the stars; but though, undoubtedly, the radiant area frequently appears to have an elongated form, further investigation does not seem to bear out that idea. Possibly the radiants M 6, 7, M 7, 8, S 5, 6, 7, 8, however, are exceptions. Signs of elongation of the radiant area

are considered by Mr. Greg, Signor Serpieri, and Professor Newton to be more or less plainly noticeable in the principal meteor-showers of August, November, and December. Some short-lived showers, or those having days of special maximum, as K 1, 3, O, L (the November shower), and Q H 2, however, appear to have a very fairly precise centre of radiation. The radiant points by Prof. Schiaparelli are those given by Dr. G. von Boguslawski, of Stettin, in his recent German publication of a new edition (considerably enlarged by the author), of Schiaparelli's work on the *Astronomical Theory of Shooting Stars.* The 189 radiants there given are, in this Table of Comparisons, reduced by grouping to 79, and the grand total of the radiants now presented is 132. It is, however, probable that a certain number of these are either pseudo-centres or require further confirmation or correction. In some cases, probably, the truest radiant-positions may be found in a more or less nearly average position of those given by two or three of the catalogues now for the first time fully and systematically tabulated and arranged together. In several cases, as with U, B 5, R G, E 2, Y, H, of Greg and Herschel, some of them representing very well-marked and important meteor-showers, the position and epoch given by Greg and Herschel are certainly more accurately placed and determined than those of their counterparts in the other lists. For certain showers, as L, A 10, Q H 1, V, T 1, Q H 2, O, G 1, K 1, 3, A G 1, S 4, S G 2, U, F 1, 2, L H, &c., there are pretty well established days of maximum frequency, giving meteors more or less characteristically distinguished from those of other showers. Some of these distinctions or peculiarities have been attempted to be given with the Atlas published by Messrs. Greg and Herschel for the British Association in 1868, but more observation specially directed to these points is required. It is yet too early to attempt to give results of a comparison of the list of radiant-points with the orbits of comets; but it is well known that some successful attempts in this direction have been already made by Schiaparelli, Weiss, Galle, Newton, and D'Arrest. The first and most important object at present attempted to be gained is to collect all the best observed radiant-positions and epochs of the meteoric showers themselves, and to ascertain, by special and further observation, with as much accuracy as possible, their true positions and durations. With the view to assist these determinations, and to enable observers to arrive at uniformity in the method and arrangement of their results, it is hoped that the accompanying Table will be found to be of some utility, as representing, principally for the northern hemisphere, the limits to which this branch of observations in meteoric astronomy may be briefly and comprehensively described as having at present reached. In most cases positive accuracy is impossible, from the very nature of the case, absolute accuracy of observation of the meteors themselves being unattainable; nor, in many cases, is it probable that the exact average centre of radiation of a number of meteors will be quite similarly determined by two different collators, even from the same series of observations.

A Synoptic or General Table of the Radiant Positions and Duration of Meteor Showers, observed in the Northern Hemisphere, 1872.

Progressive No. 1872. (R. P. Greg).	Epoch or Duration of Meteoric Shower.	Average Position of Radiant. R.A.	N.D.	Name or Number of Radiant.	Authority.*	Observations. (N.B. Radiant Area m be 15° in Diameter. Radi by Mr. Greg, from Z Observations.
1	Dec. 20 — Feb. 26	10°	86°	N G	G.&H.	Radiant elongated to 320°,+80°. Centre
	Dec. 15 — Feb. 14	222	87	N 21, 1, 2, 3	Heis	
2	Dec. 27 — Jan. 31	130	47	189, 7, 23	S. & Z.	Centre about 135°,+45 of 1680? (Weiss).
	Jan. 1 — Feb. 9	135	40	M 1, 2	G.&H.	
	Jan. 1-15	145	51	M 1	Heis	
3	Dec. 27 — Jan. 29	203	53	1, 6, 12, 13, 16, 18, 21, 188	S. & Z.	Radiant multiple.
	Jan. 6-29	200	55	Z 10	G.&H.	
4	Jan. 2-3	238	45	K 1, 3	G.&H.	Corroborated by Am servations. A notal epoch, maximum and
	Dec. 15 — Jan. 15	231	53	K 1, 3	Heis	
5	Jan. 10	10	57	3	S. & Z.	Probable centre near
	Dec. 20 — Jan. 30	21	56	A 1, 2	G.&H.	
	Dec. 15 — Jan. 31	32	56	A 20, 1, 2	Heis	
6	Jan. 11	47	40	4	S. & Z.	
7	Jan. 6	150	67	G_2	G.&H.	(Noticed by Mr. Clark 1872.) Possibly = N
8	Dec. 22 — Feb. 6?	181	35	2, 5, 30, 185, 186	S. & Z.	Radiant area large; real centre at 180°+ II. 1792? (Weiss). bably incorrect.
	Jan. 1-25	183	36	M G 1	G.&H.	
	Jan. 16 — Feb. 1	169	45	M 2	Heis	
9	Jan. 19 — Feb. 5	209	25	9, 14, 15, 27	S. & Z.	Radiant multiple?
10	Jan. 27	132	67	17	S. & Z.	Centre about 140°,+70 and rather uncertain
	Jan. 21 — Mar. 20?	140	71	M 4, 5	G.&H.	
11	Jan. 28	67	25	19	S. & Z.	Denza found 72°,+18°.
	Dec. 20? — Feb. 6	65	20	A G 1	G.&H.	
12	Jan. 3 — Mar 16	143	−7	S 1, S G 1	G.&H.	
13	Jan. 9-19	72	4	G_3	G.&H.	Reduced from Denza's by Mr Greg, and co England.
14	Jan. 19 — Feb. 6	242	63	11, 29	S. & Z.	(223°,+54°, by Mr. G Denza's observations 225°,+54°.
	Jan. 29 — Feb. 6	223	54	K 2	G.&H.	
	Jan. 15-31	227	54	K 2	Heis	

The initials S. & Z., refer to Prof. Schiaparelli and Sig. Zezioli (1867–1870); G. & H., to Mr. Greg and Prof. Alex. Herschel (British Association, 1848–1871); Heis, to Prof. Heis, of Münster (1839–1867). The Nos. under "Name or Number of Radiant," to Boguslawski's Catalogue in the translation into German of Signor Schiaparelli's work on meteors.

r Duration of c Shower.	Average Position of Radiant. R.A. °	N.D. °	Name or Number of Radiant.	Authority.	Observations. (N.B. Radiant Area supposed to be 15° in Diameter. Radiants, "Z," by Mr. Greg, from Zezioli's Observations.)
—Feb. 13	226	30	8, 10, 20, 22, 24, 25 ?, 33	S. & Z.	Radiant multiple; centre 230°, + 32°.
29	233	34	Q Z	G. & H.	
14	61	46	A 3	Heis	
	183	56	31	S. & Z.	Probable commencement of M 7 of Heis.
.8 .	172	59	M 3, 4	Heis	
	74	48	39	S. & Z.	= A Z 3; centre 73°, + 40°. Radiant precise.
7	73	40	A 3, 4	G. & H.	
-28	76	40	A 4	Heis	
15	131	52	28, 37	S. & Z.	Requires further investigation. Radiant elongated according to Schiaparelli.
15	120	54	M 5 ?	Heis	
15	50	47	A 5	G. & H.	Confirmed by Italian obs.
—Mar. 15	50	49	A 5	Heis	
	153	21	26	S. & Z.	
26	153	35	M 3	G. & H.	
— Apr. 2	175	10	S 2, 3	G. & H.	This series of radiants (S. of Heis) seen by Neumayer in Australia, probably advances from R.A. 175 to R.A. 200, with the time.
—Mar. 31	177	13	S 1, 2, 3	Heis	
-20	194	15	S 2, a	G. & H.	Possibly a continuation of No. 14.
-19	238	51	40, 41	S. & Z.	Meteors small.
	133	26	32	S. & Z.	Ditto.
.	105	62	34	S. & Z.	Ditto.
.	263	68	36	S. & Z.	Ditto.
-28	245	76	N 4 ?	Heis	
-15	209	52	35, 38	S. & Z.	Ditto.
·15	50	49	A 5 ?	Heis	
-25	247	− 3	S Z 1	G. & H.	Possibly commencement of S Z 2.
-17	190	1	S 4	G. & H.	Probable commencement of S 5, 6.
-27	74–112	32–47	A Z 1, 2, 3	G. & H.	In part = A 3, 4 radiant contd.? Small meteors obsd. by Zezioli: A Z 1 = 112°, + 32°; A Z 2 = 98, + 46; A Z 3 = 74, + 47 = A 3, 4?
o — Apr. 25	143	51	43, 61	S. & Z.	Central position 148, + 48; perhaps connected with M Z.
2 — Apr. 23	146	46	M Z	G. & H.	
6-31	150	47	M 6	Heis	
7	186	56	42 ?	S. & Z.	Probable continuation of M 3, 4 of Heis. Centre 180, + 50.
3 — Apr. 30	180	60	M 6, 7	G. & H.	
-15	180	49	M 7	Heis	

A Synoptic or General Table of the Radiant Positions and Duration of Meteor Showers, observed in the Northern Hemisphere, 1872.

Progressive No. 1872, (R. P. Greg).	Epoch or Duration of Meteoric Shower.	Average Position of Radiant. R.A.	N.D.	Name or Number of Radiant.	Authority.*	Observations. (N.B. Radiant Area supposed to be 15° in Diameter. Radiants, "Z," by Mr. Greg, from Zezioli's Observations.)
1	Dec. 20 — Feb. 26	10°	86°	N G	G. & H.	Radiant elongated 20°, +88° to 320°, + 80°. Centre 350°, + 87°?
	Dec. 15 — Feb. 14	222	87	N 21, 1, 2, 3	Heis	
2	Dec. 27 — Jan. 31	130	47	189, 7, 23	S. & Z.	Centre about 135°, + 45° = Comet of 1680? (Weiss).
	Jan. 1 — Feb. 9	135	40	M 1, 2	G. & H.	
	Jan. 1-15	145	51	M 1	Heis	
3	Dec. 27 — Jan. 29	203	53	1, 6, 12, 13, 16, 18, 21, 188	S. & Z.	Radiant multiple.
	Jan. 6-29	200	55	Z 10	G. & H.	
4	Jan. 2-3	238	45	K 1, 3	G. & H.	Corroborated by American observations. A notable meteor epoch, maximum 2nd-3rd Jan.
	Dec. 15 — Jan. 15	231	53	K 1, 3	Heis	
5	Jan. 10	10	57	3	S. & Z.	Probable centre near 22°, + 56°.
	Dec. 20 — Jan. 30	21	56	A 1, 2	G. & H.	
	Dec. 15 — Jan. 31	32	56	A 20, 1, 2	Heis	
6	Jan. 11	47	40	4	S. & Z.	
7	Jan. 6	150	67	G_2	G. & H.	(Noticed by Mr. Clark, at York, 1872.) Possibly = No. 132.
8	Dec. 22 — Feb. 6?	181	35	2, 5, 30, 185, 186	S. & Z.	Radiant area large; multiple; real centre at 180° + 35° = Comet II. 1792? (Weiss). Heis probably incorrect.
	Jan. 1-25	183	36	M G 1	G. & H.	
	Jan. 16 — Feb. 1	169	45	M 2	Heis	
9	Jan. 19 — Feb. 5	209	25	9, 14, 15, 27	S. & Z.	Radiant multiple?
10	Jan. 27	132	67	17	S. & Z.	Centre about 140°, + 70°?; diffuse, and rather uncertain.
	Jan. 21 — Mar. 20?	140	71	M 4, 5	G. & H.	
11	Jan. 28	67	25	19	S. & Z.	Denza found 72°, + 18°.
	Dec. 20? — Feb. 6	65	20	A G 1	G. & H.	
12	Jan. 3 — Mar 16	143	−7	S 1, S G 1	G. & H.	
13	Jan. 9-19	72	4	G_3	G. & H.	Reduced from Denza's obs. 1868, by Mr Greg, and confirmed in England.
14	Jan. 19 — Feb. 6	242	63	11, 29	S. & Z.	(223°, + 54°, by Mr. Greg, from Denza's observations.) Centre 225°, + 54°.
	Jan. 29 — Feb. 6	223	54	K 2	G. & H.	
	Jan. 15-31	227	54	K 2	Heis	

The initials S. & Z., refer to Prof. Schiaparelli and Sig. Zezioli (1867–1870); G. & H., to Mr. Greg and Prof. Alex. Herschel (British Association, 1848–1871); Heis, to Prof. Heis, of Münster (1839–1867). The Nos. under "Name or Number of Radiant," to Boguslawski's Catalogue in the translation into German of Signor Schiaparelli's work on meteors.

	Epoch or Duration of Meteoric Shower.	Average Position of Radiant. R.A. °	N.D. °	Name or Number of Radiant.	Authority.	Observations. (N.B. Radiant Area supposed to be 15° in Diameter. Radiants, "Z," by Mr. Greg, from Zezioli's Observations.)
	Jan. 18 — Feb. 13	226	30	8, 10, 20, 22, 24, 25?, 33	S. & Z.	Radiant multiple; centre 230°,+32°.
	Jan. 28-29	233	34	Q Z	G. & H.	
	Feb. 1-14	61	46	A 3	Heis	
4)	Feb. 6	183	56	31	S. & Z.	Probable commencement of M 7 of Heis.
	Feb. 1-28	172	59	M 3, 4	Heis	
	Feb. 16	74	48	39	S. & Z.	= A Z 3; centre 73°,+40°. Radiant precise.
	Feb. 9-17	73	40	A 3, 4	G. & H.	
	Feb. 15-28	76	40	A 4	Heis	
	Feb. 6-15	131	52	28, 37	S. & Z.	Requires further investigation. Radiant elongated according to Schiaparelli.
	Mar. 1-15	120	54	M 5?	Heis	
	Mar. 1-15	50	47	A 5	G. & H.	Confirmed by Italian obs.
	Feb. 1 — Mar. 15	50	49	A 5	Heis	
	Feb. 3	153	21	26	S. & Z.	
	Feb. 4-26	153	35	M 3	G. & H.	
1)	Feb. 10 — Apr. 2	175	10	S 2, 3	G. & H.	This series of radiants (S. of Heis) seen by Neumayer in Australia, probably advances from R.A. 175 to R.A. 200, with the time.
	Feb. 15 — Mar. 31	177	13	S 1, 2, 3	Heis	
	Feb. 11-20	194	15	S 2, a	G. & H.	Possibly a continuation of No. 14.
	Feb. 17-19	238	51	40, 41	S. & Z.	Meteors small.
	Feb. 13	133	26	32	S. & Z.	Ditto.
	Feb. 14	105	62	34	S. & Z.	Ditto.
	Feb. 14	263	68	36	S. & Z.	Ditto.
	Feb. 15-28	245	76	N 4?	Heis	
	Feb. 14-15	209	52	35, 38	S. & Z.	Ditto.
	Mar. 1-15	50	49	A 5?	Heis	
	Mar. 3-25	247	− 3	S Z 1	G. & H.	Possibly commencement of S Z 2.
47)	Mar. 5-17	190	1	S 4	G. & H.	Probable commencement of S 5, 6.
	Mar. 9-27	74–112	32–47	A Z 1, 2, 3	G. & H.	In part = A 3, 4 radiant contd.? Small meteors obsd. by Zezioli: A Z 1 = 112°,+32° A Z 2 = 98,+46 A Z 3 = 74,+47 = A 3, 4?
	Mar. 20 — Apr. 25	143	51	43, 61	S. & Z.	Central position 148,+48; perhaps connected with M Z.
	Mar. 22 — Apr. 23	146	46	M Z	G. & H.	
	Mar. 16-31	150	47	M 6	Heis	
17)	Mar. 17	186	56	42?	S. & Z.	Probable continuation of M 3, 4 of Heis. Centre 180,+50.
	Mar. 3 — Apr. 30	180	60	M 6, 7	G. & H.	
	Apr. 1-15	180	49	M 7	Heis	

Progressive No. 1872. (R. P. Greg.)	Epoch or Duration of Meteoric Showers.	Average Position of Radiant. R.A. °	N.D. °	Name or Number of Radiant.	Authority.	Observations. (N.B. Radiant Area supposed to be 15° in Diameter. Radiants, "Z," by Mr. Greg, from Zezioli's Observations.)
35 (50)	Apr. 1-23	256	43	46, 49, 50, 51, 57, 60	S. & Z.	Multiple radiant. Possibly commencement of D G 2. General centre of D G 1, D G 2 = 268, + 51? (See No. 50).
	Mar. 11 — Apr. 23	267	53	D G 1	G. & H.	
36	Mar. 27 — Apr. 30	224	38	44, 48, 54, 65	S. & Z.	
	Mar. 12 — Apr. 30	223	40	M G 2	G. & H.	
37	Mar. 15 — Apr. 20	305	27	W Z	G. & H.	
38	Apr. 25	260	24	63	S. & Z.	Prof. Herschel, 13 April, 1864, = 273°, + 25½° = Comet I. 1861? (Weiss). Max. 13th April. R. P. Greg, 20th April, 1872, 267° + 25°
	Mar. 15? — Apr. 23	268	25	Q H 1	G. & H.	
39	Mar. 19? — Apr. 22	277½	34½	Q H 2	G. & H.	Prof. Serpieri (267°, + 35°). Prof. Galle (278°, + 34½°) = Comet I. 1861? (Weiss and Galle). A notable shower; max. display April 19-22. Q H 1, Q H 2 and C, probably all identical.
	Apr. (20) 15-31	277	38	C	Heis	
40	Mar. 20 — May 29	227	−5	S G 2 (S Z 2)	G. & H.	
41	Mar. 25 — Apr. 30	290	−10	O Z	G. & H.	More observations required.
42	Apr. 11-29	187	24	53, 64	S. & Z.	
	Mar. 25 — Apr. 24	198	32	M G Z	G. & H.	
	Apr. 1-30	192	18	S 4, 5	Heis	
43	Mar. 30 — Apr. 14	210	54	45, 47, 55, 59	S. & Z.	Prof. Serpieri, 1869 (202 + 62). This radiant appears to advance with the time, and is a continuation of M 6, 7; lasting ten weeks?
	Apr. 25 — May 25	202	52	M 7, 8?, M 6, 7 continued	G. & H.	
44	Apr. 10-14	165	47	52, 56	S. & Z.	
	Apr. 15-30	160	49	M 8	Heis	
45	Apr. 14	240	55	58	S. & Z.	
46	Apr. 25	256	75	62	S. & Z.	
	Apr. 10 — May 4	70	87	N 8?	G. & H.	Radiant, well defined near *Polaris.*
	Apr. 1-30	267	84	N 7, 8	Heis	
47 (31)	Apr. 2 — May 4	194	9	S 5, 6	G. & H.	Radiant elongated, 189° to 200°, R. A.; and 5° to 12° Dec. = S. 5, 6, 8?
48	Apr. 29 — June 12	123	40	M G 4	G. & H.	
49 (53)	Apr. 19-20	152	22	M 3 Z	G. & H.	(Perhaps commencement of Y?) Elongated radiant
50 (35)	May 25	280	54	74	S. & Z.	Well-defined radiant and shower; continuation of D G 1 possibly.
	Apr. 23 — May 31	270	55	D G 2	G. & H.	
51	May 22 — July 5	240	24	70, 83, 87	S. & Z.	A well-marked shower. Radiant centre at 239° + 23°, and probably multiple. Duration nine weeks. β *Herculis* in April; and possibly distinct from position in corona for June.
	Apr. 12 — June 30	240	25	Q 1, 2	G. & H.	
	May 1 — June 30	237	19	Q 1, 2	Heis	
52	Apr. 27 — June 30	305	81	N 9, 10	G. & H.	Radiant well defined.
	May 1-31	236	81	N 9, 10	Heis	

	Epoch or Duration of Meteoric Shower.	Average Position of Radiant. R.A. °	N.D. °	Name or Number of Radiant.	Authority.	Observations. (N.B. Radiant Area supposed to be 15° in Diameter. Radiants, "Z," by Mr. Greg, from Zezioli's Observations.)
9)	Apr. 29 — May 15	163	12	Y	G. & H.	(= M 3, Z ?) Radiant elongated, 160° to 167° R.A., and 75° to 70° Dec.
7)	Apr. 2 — May 4	195	7	S 5, 6, 8	G. & H.	Probably a continuation of S 4, 5. Radiant elongated?
	May 1-31	202	9	S 6	Heis	
2)	May 1-31	325	55	B 1	Heis	Probably commencement of No. 62 or 65.
	May 18 — June 14	273	34	68 (?), 75, 79	S. & Z.	Radiant elongated? centre at 270° + 32 (No. 68 doubtful.)
	May 6 — June 30	280	29	W	G. & H.	
	May 2 — June 20	312	21	W G	G. & H.	Radiant elongated.
	June 28	302	27	82 ?	S. & Z.	
	May 2 — June 9	206	39	66, 72, 78	S. & Z.	Small meteors; probably a multiple radiant.
	May 16 — June 2	235	43	67, 71, 77	S. & Z.	Ditto.
	May 22-24	301	37	69, 73	S. & Z.	Ditto.
	May 26	237	59	76	S. & Z.	Ditto.
5)	June 1-30	333	42	B 2	Heis	(= B 1 of Heis continued?) Doubtful.
	June 7 — Aug. 12?	294	3	Q G	G. & H.	= Neumayer and Heis Ξ radiant? Radiant, well defined at 294° + 7°. Ξ 2 for July at 305 + 5.
	June 1-30	292	15	W	Heis	
	June 1-29	168	55	M G 3	G. & H.	Radiant rather uncertain, more observations needed.
	July 18-31	320	62	94 ?, 101, 102, 107, 114 ?, 127	S. & Z.	Multiple radiant. Radiant precise.
	June 11 — July 11	315	60	B 1	G. & H.	
	July 1-31	317	62	B 3, 4	Heis	
	June 28 — Aug. 3	270	51	81, 90, 95, 97, 121, 131	S. & Z.	Confirmed in 1872 by Miss Herschel at 274°, + 57°.
	June 29 — Aug. 24	330–345	14	T 1	G. & H.	Requires further investigation; and possible connexion with Nos. 76 and 88. At 345 + 15 in 1871, A. S. Herschel. Radiant elongated.
0)	June 19 ? — Aug. 4	304	40	80 ?, 86, 88, 92, 109 ?, 112, 116, 117, 120, 126, 133	S. & Z.	
	July 10 — Aug. 20	310	47	B 4	G. & H.	Heis B. 6. doubtful. Clark at York gives the position at 315°, + 42°.
	Aug. 15-31	304	59	B 6 ?	Heis	
	July 4	3	68	84	S. & Z.	Radiant diffuse; elongated?
	July 7 — Aug. 4	12	70	N 11	G. & H.	
	July 5	222	60	85	S. & Z.	Serpieri; 11 July, 1868 = 200°, + 55°, centre 216°, + 57°? Confirmed by Zezioli's observations.
	July 1-11 ?	210	55	M G 5	G. & H.	
	July 2 — Aug. 16?	280	65	B 3	G. & H.	Heis, position doubtful.
	July 8	288	64	89	S. & Z.	
	Aug. 1-15	297	68	B 5	Heis	

Progressive No. 1872. (R. P. Greg).	Epoch or Duration of Meteoric Shower.	Average Position of Radiant. R.A.	N.D.	Name or Number of Radiant.	Authority.	Observations. (N.B. Radiant Area supp be 15° in Diameter. Radian by Mr. Greg, from Zezi Observations).
72 (51)	July 4 — Sept. 12	248°	18°	122, 124	Q. 3	Radiant rather diffuse, a gated? Heis incorrect
	July 1-15	262	12	Q 3	Heis	
73	July 4-11?	210	20	Q 4	G. & H.	Confirmed also by Zeziol and reduced by Mr. Gr
74	July 16	257	36	Q H	G. & H.	Observed by Prof. Hers July, 1871. Perhaps=
75 (93)	July 24 — Aug. 11	330	88	115, 135?, 143	S. & Z.	Radiant well defined, clo *loris*. At 10° + 83 in th
	July 28 — Sept. 10	359	90	N 12, 13	G. & H.	
	July 1 — Aug. 31	220	85	N 11, 12, 13, 14	Heis	
76	July 18 — Aug. 4	344	40	98, 100, 103, 104, 106, 113, 123, 134	S. & Z.	Radiant multiple. Co group near η *Pegasi*; at in 1872.
	Aug. 3-15	337	25	T G	G. & H.	
77	July 2 — Aug. 16	342–34		H	G. & H.	= Y_1 of Neumayer an Southern Hemisphere near *Fomalhaut*, obse A. S. Herschel, 28 Ju Also seen in England, A 1871.
78	July 11 — Aug. 7	7	50	93, 111, 129, 130, 138	S. & Z.	From Zezioli's obs. (A. 9 sibly with R 1, 2 of G.
	July 4-11?	7	42	A 9	G. & H.	
79	July 28	174	55	118	S. & Z.	A well-defined radiant teoric shower, 1869-71. mum about 10th of Au
	July 29 — Sept. 6?	165	53	V	G. & H.	
80 (68)	July 21	309	40	109?	S. & Z.	Possibly connected with
	July 4 — Aug. 22	315	31	B G	G. & H.	
81	Aug. 15-31	314	15	T 1	Heis	Heis, position probably in
82	July 30 — Aug. 4	33	34	125, 132	S. & Z.	
83	July 9-21	242	68	91, 96, 108?	S. & Z.	Radiant well defined.
	July 12-31	245	64	B Z	G. & H.	
84	July 21	11	38	110	S. & Z.	Requires further inve as to Epoch and durat
	July 28 — Sept. 3?	1–15	36	R 1, 2	G. & H.	
85	Aug. 6-12	45	51	137, 139, 142, 144	S. & Z.	N.B. The Perseids have gated radiant area e from *Perseus* to *Ca* = Comet III. 1862.
	Aug. 10-12	50–30	49–64		Serpieri	
	July 28 — Aug. 15	50–25 44	50–65 56	A 10	G. & H.	
	July 15? — Aug. 15	51	55	A 10, 11	Heis	
86	Aug. 6	254	37	136	S. & Z.	
87	Aug. 10	47	18	140	S. & Z.	
88	Aug. 10-11	3	17	(Tacchini)	S. & Z.	Possibly a pseudo-radian and Perseids; or a con ment of T 2, 3.
	Aug.	355	18	T 1 *a*	G. & H.	
89	Aug. 22 — Oct. 15?	359	17	T 2, 3, (4?)	G. & H.	= Σ_2 of Neumayer and 346 + 3 for Sept.
	Sept. 1-30	352	10	T 2, 3	Heis	

Epoch or Duration of Meteoric Shower.	Average Position of Radiant. R.A.	N.D.	Name or Number of Radiant.	Authority.	Observations. (N.B. Radiant Area supposed to be 15° in Diameter. Radiants, "Z," by Mr. Greg, from Zezioli's Observations.)
	°	°			
Aug. 28	340	65	94, 145	S. & Z.	Exact duration uncertain. Radiant precise. Perhaps connected with No. 65 or 96.
Aug. 6-31?	335	67	E 1	G.&H.	
Aug. 2 — Sept. 25	285	44	B 5	G.&H.	Well-marked shower; radiant well defined.
July 29-31	276	36	122, 124	S.&Z.	
Aug. 15 — Sept. 30	38	63	A 12, 13, 14	Heis	G. Forbes, Cambridge, Aug. 10, 1868 = 33° + 59°.
5) Sept. 19 — Oct. 20	10	88	N 14	G.&H.	Probably continuation of N 12, 13.
Sept. 1 — Oct. 15	100	85	N 15, 16, 17	Heis	
Sept. 6 — Oct. 12	55	33	147, 150, 155	S. & Z.	
Oct. 14 — Nov.?	46	27	R 3?	G.&H.	
Sept. 1 — Oct. 15	48	34	R 1, 2, 3	Heis	
Sept. 1 — Oct. 15	306	62	B 7, 8, 9	Heis	
Sept. 5-20	319	53	146, 151	S. & Z.	Epoch requires further investigation, perhaps = No. 95. A well-marked shower in September.
Aug. 10 — Sept. 30?	335	52	E 2	G. & H.	
Sept. 7-12	60	65	148, 149	S. & Z.	
Sept. 23	28	35	152	S. & Z.	
Sept. 6 — Nov. 23?	17	10	U	G.&H.	A southern radiant. Well observed 27th Sept. 1864, by A. S. Herschel, in Kent.
Sept. 28 — Nov. 10	82	49	153, 162, 166	S. & Z.	Multiple radiant; centre about 82° + 50°. An important and well-defined meteoric shower, with tendency to advance with the time from 75° + 45° to 90° + 54°.
Sept. 17 — Nov. 24	83–92	50–55	F 1, 2	G.&H.	
Oct. 16-31	72	44	A 16	Heis	
Oct. 1-15	57	61	A 15	Heis	
Oct. 5	240	63	154	S. & Z.	
Oct. 21	130	48	159	S. & Z.	
Oct. 3-20	142	44	L G	G.&H.	
Oct. 13-21	84	21	156, 157, 158, 160	S. & Z.	Tacchini, 21st Oct. 1867; radiant 74° + 25°. Maxm. 18-21 Oct. A well-marked shower; radiant precise at *ν Orionis*. Maxm. 18-20 Oct.
Oct. 17 — Nov. 13?	90	15	O	G.&H.	
Oct. 23	111	29	161	S. & Z.	
Oct. 28	110	70	163	S. & Z.	
Oct. 16-31	205	85	N 18	Heis	
Oct. 16-31	334	56	B 10	Heis	
Oct. 16-31	23	40	P 1	Heis	A doubtful radiant.
Oct. 18-29	283	43	B G 6	G. & H.	More observations required for this shower.
Nov. 10	70	20	165	S. & Z.	Mr. Backhouse 4-6 Nov. 1869, radiant = 54° + 16°; also observed at Greenwich 13th Nov. 1870 at 55° + 25°. Generally a well-marked shower at *α Tauri*.
Oct. 25 — Nov. 21	64	18	R G 2	G. & H.	
Nov. 1-15	55	16	R 4	Heis	

Progressive No. 1872. (R. P. Greg.)	Epoch or Duration of Meteoric Shower.	Average Position of Radiants. R.A.	N.D.	Name or Number of Radiant.	Authority.	Observations. (N.B. Radiant Area supposed to be 15° in Diameter. Radiants, "Z," by Mr. Greg, from Zezioli's Observations.)
		°	°			
112	Oct. 31 — Dec. 12	134	6	L H	G. & H.	
113	Nov. 9	61	42	164	S. & Z.	
114	Nov. 12-13	359	37	168, 170	S. & Z.	
115	Nov. 13-15	149	23	L	G. & H.	Leonids identical with Comet I. 1866.
	Nov. 13-15	148	24	L	Heis	
116	Nov. 23 — Dec. 9	291	53	D G 3	G. & H.	
	Nov. 1-15	279	56	D ?	Heis	
117	Nov. 13-14	33	40		Denza	Observed by Prof. Denza, 1870 (Query pseudo radiant of = A 16, P 2, 3 or R 3 ?)
118	Nov. 13-14	40	60	171	S. & Z.	Connected with R 3? No. 171 probably a pseudo-radiant. Requires further investigation.
	Nov. 23 — Dec. 18	45	55	A 16	G. & H.	
	Oct. 16 — Nov. 30	46	44	P 2, 3	Heis	
119	Nov. 12-17	117	38	169, 172	S. & Z.	
120	Nov. 30	17	48	176	S. & Z.	Radiant probably elongated. Supposed by D'Arrest and Weiss to be connected with Biela's Comet.
	Sept. ? — Nov. 25 ?	7–23	54–61	A 14, 15	G. & H.	
	Nov. 1 — Dec. 15	17	60	A 17, 18, 19	Heis	
121	Nov. 10 — Dec. 9	142	36	167, 173, 177, 178, 181	S. & Z.	
122	Nov. 13	193	40	No. 122	G. & H.	Observed at Greenwich, 1870; radiant near *Cor Caroli.* A doubtful radiant.
123	Dec. 23	157	64	187	S. & Z.	Radiant elongated 140°, + 70° to 170°, + 72°. Shower well marked.
	Nov. 4 — Dec. 19	160	71	K G	G. & H.	
124	Nov. 19 — Dec. 15	123	84	N 19, 20	Heis	
125	Nov. 23-26	103	32	174, 175	S. & Z.	An important meteoric shower; maxm. 11th Dec. Radiant centre δ *Geminorum.* Radiant at 105° + 30°, 12th Dec. 1861; R. P. Greg and A. S. Herschel. Mr. Wood at 100° + 34° in 1863. Probably elongated 90° + 40° to 105° + 28°. Aërolitic epoch.
	Nov. 26 — Dec. 30	100	33	G_1	G. & H.	
	Dec. 1-15	112	39	M 9	Heis	
126	Dec. 9	154	26	182	S. & Z.	
127	Dec. 9-21	108	63	179, 180, 184?	S. & Z.	
128	Dec. 12	180	53	183	S. & Z.	
129 (7)	Dec. 23	157	64	187	S. & Z.	Possibly commencement of No. 7. (Jan. 6.)
				? G_2	G. & H.	

Supplementary Radiants.

130	Aug. 1-31	337	−10	Σ 2	Heis	A southern radiant in the list of Heis and Neumayer; observed in England 9-12 Aug. 1871.
131	Aug. 3-30	68	46	F G	G. & H.	Reduced from the observations of the Radcliffe Observatory at Oxford in 1870-71. The Radiant V also observed simultaneously.
132	Aug. 3-15	55	26	R G 1	G. & H.	

In making or reducing meteor observations, note on a single night, or on two or three consecutive evenings, it is always best to map the results *at once*, and either to endeavour to determine any new radiant, or to correct the position of old-established ones; making use of this catalogue as a guide, rather than as an end. There being doubtless a tendency for particular meteor-showers to appear most strongly marked on certain special dates, it will thus be possible to determine the position of their radiant points more accurately than they are frequently at present known. Observations specially directed towards the supposed position of a radiant will also best show the shorter tracks, which are best calculated to determine the precise position of the radiant point of any particular meteor-shower: and the characteristic features of speed or duration, brightness, colour, and of the attendant sparks or streak of meteors belonging to different showers should at the same time be very carefully recorded, and considered in distinguishing the radiant points from which they probably diverged. The radiant V, and probably also T G (No. 78), seems to have appeared somewhat prominently only during the last two or three years; whilst the neighbouring radiant T 1 (No. 67), formerly a prominent feature, especially during the August period, and giving meteors considerably less rapid in their flight than the Perseids, have been less noticeable. Those meteor-showers, at present most clearly marked and contemporaneous with the greater 7–12th August period, are V; T G (78); N 12, 13; E 1 (90); B 3; H (77); B 4 (68); T 1 (67); and in mapping the meteors at that period it will be well to bear those radiants in mind whilst attempting to fix with any degree of precision the true radiant centre (probably in itself not a fixed or precise one) of the Perseids themselves. The series of well-marked and nearly contemporaneous meteor-showers from July to September, with radiant centres some 15°–20° around, is near *α Cephei*, including E 1; E 2; B 1, and B 4 of Greg and Herschel; B 3, B 4, and B 5 of Heis, and mostly corroborated by the Italian observers, are deserving of more study, whether as to precise duration or exact position of radiant centres and connexions with each other; or as appertaining to a single multiple radiant, or to several distinct centres of radiation.

R. P. G.

Observations of Meteor Showers, supposed to be connected with Biela's Comet. By Prof. A. S. Herschel, F.R.A.S.

In a recent publication by Dr. Weiss* on the lately discovered connexion between the appearances of meteoric showers and the orbits of certain comets, a suggestion of some importance to observers of shooting stars is offered, to which the present year

* *Beiträge zur Kentniss der Sternschnuppen;* Vienna Acad. Sitzungsbericht, vol. lvii. 1868.

affords a most suitable opportunity for directing their attention. It is well known that on the occasion of its perihelion return in the year 1832, it was pointed out by Olbers that the nucleus of Biela's comet would pass within 20,000 miles of the Earth's orbit, at a point which the Earth would occupy on about the 3rd of December in that year. Had the comet passed its descending node on that date, instead of actually about one month earlier, some particles of its substance would doubtless have entered into collision with the Earth's atmosphere, and would probably have given rise to a considerable meteoric shower. The direction of their motion relatively to the Earth (or the radiant point of the cometary shower) would, it is calculated by Dr. Weiss have been, in that case, from a point in about R.A. 23°, N. Decl. 47°; and this point would have appeared to be the centre of divergence of a shower of shooting stars. It is remarkable that a meteoric shower having nearly this radiant point has been repeatedly observed since the earliest recorded appearance of Biela's Comet, on the nights of the 6th or 7th of December, of whose appearance previously to that time no record is known to exist, although a search for its occurrence was made by Prof. Newton in the best, and most extensive catalogue of meteor-showers of earlier dates.* Its first, and apparently most brilliant display was observed by Brandes, in Germany, in the year preceding the appearance of the great November star-shower recorded by Humboldt and Bonpland in America at the end of the last century, of which shower, to judge from the description of it given by Brandes, it was, indeed, by no means an insignificant precursor. It was afterwards reobserved on the same date (December 7th) in the year 1830, in France; and again in France, Belgium, and the United States in 1838, the meteors being on this occasion about four times as frequent as on an ordinary night; and by Prof. Heis at Aix-la-Chapelle in 1847, since which time no distinct account of its reappearance on the same dates seems to have been recorded. The date of its appearance in each of the years 1838 and 1847, was on the evening of the 6th of December.

At its return in 1838, the position of its radiant point was fixed by Herrick, in the United States, as in some part of *Cassiopeia's* Chair; by M. Flaugergues in France, somewhat south of this, in *Andromeda;* and by M. Bouvy, at the observatory at Brussels, near the zenith, which was there situated at the time of the observation between *Cassiopeia* and *Andromeda.* Its approximate position, as noted by Prof. Heis at Aix-la-Chapelle, in 1847, was at a point in R.A. 25°, N. Decl. 40°, not far from the star of *γ Andromedæ.* Later observations appear to have led Prof. Heis to adopt a more northerly position (A 19 in his later list of radiant points for December) at R.A. 21°, N. Decl. 54°. This shower is therefore as totally distinct, both in its date and comparatively rare returns, as in the position of its radiant point from the far older, annually recurring, and

* *American Journal of Science,* Second Series, vol. xxxvi., p. 145, *et seq.*

better known meteor shower of the 12th of December, whose radiant point has recently been determined with certainty and precision in the neighbourhood of *θ Geminorum*, in R.A. 105°, N. Decl. 30°. As shown by Prof. Newton in his already mentioned research, the earlier appearances of this shower also extend backwards as far as the year A.D. 901.

A radiant point of several shooting stars, observed by Mr. Zezioli in Italy on the evening of November 30, 1867, is contained in Prof. Schiaparelli's list of radiants derived from Mr. Zezioli's observations (as shown in Mr. Greg's comparative table) at R.A. 17°, N. Decl. 48°, near *θ Cassiopeiæ*. An elongated region of radiation of shooting stars, lasting for a long period until near the end of November (A 14, 15 in the English list), is there also placed by Mr. Greg close to the position of the radiant A 19 of Prof. Heis, although preceding it considerably in point of time. But it appears not impossibly to be connected by intermediate meteors with the succeeding radiant A 16 in the same list, whose position is somewhat too distant to be included in the former group. The node of the orbit of Biela's comet possessing an extremely rapid retrograde motion, so that the date of the Earth's passage through it now falls in the end of November instead of in the beginning of December, it appears probable that the dates of the last two meteoric showers, although not coinciding with the 6th or 7th of December, may yet have belonged to returns of the cometary meteor group. The following longitudes and dates of the Earth's passage through the node, and the corresponding positions of the cometary radiant point from the earliest to the latest recorded appearances of Biela's comet, were calculated by Dr. Weiss from Hubbard's elements of the comet's orbit at those times of its appearance. The mean motion of the node of a meteoric cloud revolving in the same orbit as that of Biela's comet, arising from the planetary perturbations, if calculated in the same manner for the 7th of December meteors as that calculation was successfully accomplished by Prof. Adams for the December meteor group, would indicate, as suggested by Dr. Weiss, the extent to which such a meteor cloud may be expected to participate with Biela's comet in the rapid retrograde displacement of its node.

Year of Observation.	Longitude of Desc. node. (1850·0).	Date of Earth's Passage through Node.	Position of Cometary Radiant Point. R.A.	N. Decl.	Velocity of the Meteor relatively to the Earth.
	°		°	°	
A.D. 1772	258·4	Dec. 10	18·7	58·1	9·6 miles per second.*
„ 1826	251·8	Dec. 4	22·8	47·7	9·6 „ „
„ 1852	245·8	Nov. 28	23·4	43·0	9·7 „ „

* These velocities are without allowing for the effect of the Earth's attraction. When this is taken into consideration the relative velocity of the meteors of Biela's comet is found to be 11·9 miles per second, while that of the Leonids, or meteors of the November shower, is 44·3 miles per second.

Owing to the comet's motion in its orbit, and that of the Earth being in the same direction, and but slightly inclined to each other (about 22°), at their point of nearest approach, the case of this meteor stream overtaking the Earth is a somewhat extreme example of the opposite kind to that of the November meteor current, which meets the Earth with a small inclination to its course, moving in an opposite direction. The speed with which the meteor particles of Biela's Comet enter the Earth's atmosphere (allowing for the effect of the Earth's attraction) is thus little more than a fourth part of that with which the November meteors first penetrate it. Their radiant point is, for the same reasons, more westerly than those of ordinary meteor showers, arriving near the zenith at Greenwich soon after eight o'clock in the evening, and descending towards the north-west and north horizon (where it is circumpolar) during the later hours of the night. In its approach to the northern horizon (should the meteors be observed in the early morning hours), the effect of the Earth's attraction upon their nearly horizontal courses, owing to the extremely low relative velocity presented in the case of this westerly meteor current, is so considerable that the apparent radiant point is raised about 10° from the apparent position which it has when the radiant point of the shower is in the zenith.

Besides the disruption of Biela's comet into two parts, which was witnessed during its reappearance in the year 1832, it is observed by Dr. Weiss that the approximate orbit of the telescopic Comet I. 1818, appears to coincide so nearly with that of Biela's comet, that should it be a fragment of that comet formerly separated from its principal body, and now revolving in nearly the same periodic time with it, but passing its perihelion somewhat more than one year earlier than Biela's Comet, the star-showers of December 1798 and 1838, would be found to coincide with the dates of perihelion passage of this companion comet at the beginning of those years in the same way that the great star-showers of August 1863, and November 1866, followed quickly after the appearances of the Comets III. 1862, and I. 1866. In the following list of the approximate dates of the former and present passages of Biela's comet (and of Comet I. 1818) through its node, it will be seen that one of the December meteor-showers was similarly observed in 1847, following one of the nodal passages of Biela's comet in the preceding year; and that the shooting-stars from *Cassiopeia*, observed by Sig. Zezioli in Italy on the 30th of November, 1867, appeared in the year following the return of Biela's Comet to its node in the year 1866.

Should a clear sky permit a watch to be kept for these meteors during the last week in November, and the first week in December next, and especially on the nights of the 4th to the 7th of December, both in this and in next year, a favourable opportunity for verifying practically these ingenious suggestions of Dr.

...sents itself in the occasion of the present return of ...t, which it may be hoped will not escape the atten...ners, nor pass unnoticed by observers and recorders ...rs.

...rvation of the Great Meteor Shower ...6th and 7th of December.	Approximate Dates of the Passage of Biela's comet (and of Comet I. 1818) through its Node. A.D.
...r of meteors observed by ... Germany. Four hundred ... few hours (in half of the ...ing). Benzenberg, *Die Stern-*	1799·3 (1798·1; Comet I. 1818?).
... Many shooting stars seen in France, by ...bbé Raillard. *Comptes Rendus*, 1839, Feb. 4.	1832·8 (1831·4; Comet I. 1818?).
..838, Dec. 6 (P.M.). Meteor shower observed in France, at Toulon, by Paul Flaugergues; and in the United States by E. C. Herrick. Seen also at Brussels, by M. Bouvy, Assistant at the Observatory, from 7^h 30^m P.M. Meteors four times as frequent as on an ordinary night, descending from *Pegasus* and *Aries* towards the S.E. and S.W. horizon from the direction of the zenith, [*i.e.*, from near *Cassiopeia*; radiant in *Cassiopeia*.] *Comptes Rendus*, 1839, Jan. 21, and Feb. 4 (*Bulletins*, Ac. Roy. de Belgique, vol. vi., No. 6, June, 1839).	1839·4 (1838·1; Comet I. 1818?).
1847, Dec. 6 (P.M.). Shower of meteors observed by Prof. Heis, at Aix-la-Chapelle. Radiant point at R.A. 25°, N. Decl. 40° [near γ *Andromedæ*.] Heis, *Die Periodische Sternschnuppen*, Cöln, 1849.	1846·1 (Biela's comet separated into two parts.
1867, Nov. 30 (P.M.). Nine meteors in two hours recorded by M. Zezioli at Bergamo, Italy; all radiating nearly from a point in R.A. 17°, N. Decl. 48°, near θ *Cassiopeiæ*. Prof. Schiaparelli; Preliminary results of Shooting-star observations made by Zezioli, Part II. *Effemerides Astronomiche del Osservatorio di Milano*, 1871.	1866·0 (Biela's comet not seen since 1852. Present nodal passage of [Biela's comets 1872·7; of Comet I. 1818, 1871·4?).

Dr. Huggins's Spectroscopic Observations of Stars and Nebulæ.

Dr. Huggins has communicated to the Royal Society a series of results of extreme interest, obtained by means of the fine telescope placed at his disposal by the Royal Society.

His work has been divided into two main portions. First, he has been engaged in comparing one of the bright spectral lines of

the gaseous nebulæ with the corresponding line in the spectrum of nitrogen. This line, as seen in the latter spectrum, is double. When using his own 8-inch telescope, Dr. Huggins was unable to determine whether the nebula line was double or not. He could not use sufficient dispersive power. He has now obtained definite results on this point, so far at least as the *Orion* nebula is concerned. In the spectrum of nitrogen the components of this double line are rather broad and nebulous. In the spectrum of the *Orion* nebula there is one line, narrow and well defined, which agrees in position with the less refrangible of the nitrogen pair. There are two available explanations of this. It may be that under certain conditions the line becomes single, occupying a position midway between the positions of the two lines, and that in the spectrum of the *Orion* nebula the line is shifted owing to the motion of the nebula. A motion at the rate of from 30 to 40 miles from the Earth would account for the displacement. Or again, it may be that under certain conditions the more refrangible line might fade out altogether. Dr. Huggins has tried the experiment, but has not been able to reduce the spectrum of nitrogen to a single line. The observations required are somewhat difficult, as the lines fade out under the change of condition which alone seems to promise to effect the simplification of the spectrum. But Dr. Huggins feels tolerably certain that the line is always double, and that there is no change in its position.

It is possible, however, Dr. Huggins remarks, that this line in the spectrum of the gaseous nebulæ is not due to nitrogen at all. Or else, one line of nitrogen fades out altogether. He proposes to compare the spectrum of nitrogen with the spectra of other gaseous nebulæ.

The second series of results obtained by Dr. Huggins relates to the determination of the stellar motions of recession or approach. In making these observations, Dr. Huggins has not trusted to the ordinary method of comparing spectra by placing a prism over one half of the slit, and so bringing the spectrum of comparison beside that of the star under examination. He points out that this method is in several respects unsatisfactory. Indeed, he did not trust solely to this method in his former observations of *Sirius*. He set a light directly opposite the object-glass of his telescope, and compared its spectrum with that of the star. But with a telescope 15 feet long, like that which he is now employing, this arrangement is not convenient, and the light is rendered by distance too faint for his purpose. He has therefore had holes drilled in the telescope tube, 30 inches from the focus, and causes a holder containing a vacuum tube to be placed exactly in the optical axis of the telescope. Instrumental means exist for accurately adjusting the vacuum tube in this position; but the accuracy of the adjustment can also be very readily tested.

Dr. Huggins had judged when he used his 8-inch telescope that *Sirius* is receding at the rate of about 25 miles per second.

He now finds that the rate of recession is somewhat less, lying probably between 18 and 22 miles per second.

He has been able to extend this method to several other stars. He finds evidence in favour of a general tendency to recession in stars occupying that part of the heavens from which our Sun is known to be travelling; while on the opposite side of the heavens the stars seem in general to be approaching. But the rates of recession and approach accord very ill with the usually adopted value of the solar proper motion, and appear to support the theory recently advanced, that the estimates of the stellar distances, on which that value has been based, are not trustworthy. We know that the Sun's rate of motion has been set at five or six miles per second, and such a rate of motion could only account for a general excess of recession in stars lying in one direction, and of approach in stars lying in the opposite direction, by about the same amount. But Dr. Huggins finds motions of recession of from 15 to 28 miles per second, and motions of approach amounting even to the enormous rate of 55 miles per second in the case of *Arcturus*. It would seem to follow from this that Struve's estimate of the average distances of the brighter stars is altogether too low.

The following tables are from Dr. Huggins's paper:—

Table I. Stars moving from the Sun.

Star.	Compared with	Apparent Motion.	Earth's Motion.	Motion from Sun.
Sirius	H	26 to 36	−10 to −15	18 to 22
Betelgeux	Na	37	−15	22
Rigel	H	30	−15	15
Castor	H	40 to 45	−17	23 to 28
Regulus	H	30 to 35	−18	12 to 17
β Urs. Maj.	H			
δ —	H			
ε —	H	30	−9 to −13	17 to 21
γ —	H			
ζ —	H			
β Leonis	H			
δ —	H			
η Urs. Maj.	H			
α Virginis	H			
α Coronæ B.	H			
Procyon	H			
Capella	H			
Aldebaran (?)	Mg			
γ Cassiopeiæ	H			

Table II. Stars approaching the Sun.

Star.	Compared with	Apparent Motion.	Earth's Motion.	Motion towards Sun.
Arcturus	Mg	50	+ 5	55
Vega	H	40 to 50	+ 3·9	44 to 54
α Cygni	H	30	+ 9	39
Pollux	Mg	32	+17	49
α Ursæ Maj.	Mg	35 to 50	+11	46 to 61
γ Leonis	Mg			
ε Bootis	Mg			
γ Cygni	H			
α Pegasi	H			
γ Pegasi (?)	H			
α Andromedæ	H			

It will be seen that the five stars β, γ, δ, ε, and ζ, are all receding at the rate of about 30 miles per second. Moreover, Dr. Huggins finds that the spectra of all these stars present similar characteristics, the lines of hydrogen being very broad and strong. In η *Ursæ Majoris* the line of hydrogen is not so strong or so broad, nor is η receding at the same rate; so that clearly this star does not belong to the same set. The star α *Ursæ* is found to have a spectrum markedly different from that common to β, γ, δ, ε, and ζ, and to be approaching the Earth instead of receding. This agrees with the theory recently advanced, that the five stars, β, γ, δ, ε, and ζ, form a drifting group of stars.

Sweeping Ephemerides for the Comet of Biela, including the Period of Absence of Moonlight in the Morning Sky in October. By J. R. Hind, Esq.

1872. $9^h\ 36^m$ G.M.T.	Perihelion, Sept. 28·4. R.A.	Decl.	Perihelion, Oct. 6·4. R.A.	Decl.	Δ	Perihelion, Oct. 14·4. R.A.	Decl.
	h m	° ′	h m	° ′		h m	° ′
Sept. 28	10 4·0	+6 27	9 43·0	+9 15	1·345	9 20·3	+12 22
30	13·0	5 21	9 52·4	8 6		30·2	11 9
Oct. 2	21·9	4 15	10 1·7	6 56	1·365	40·0	9 56
4	30·7	3 9	10·9	5 46		49·7	8 42
6	39·3	2 4	19·9	4 37	1·388	9 59·2	7 28
8	47·8	+1 0	28·8	3 28		10 8·6	6 15
10	10 56·2	−0 3	37·6	2 20	1·414	17·8	5 2
12	11 4·5	1 4	46·2	1 13		26·9	3 50
14	12·6	2 5	10 54·8	+0 7	1·442	35·8	2 38
16	20·6	3 4	11 3·2	−0 58		44·6	1 27
18	11 28·5	−4 2	11 11·4	−2 2	1·472	10 53·3	+0 18

Mr. Bishop's Observatory,
Twickenham, 1872, *Sept.* 17.

Discovery of two New Minor Planets (122) *and* (123).

By Prof. C. H. F. Peters.

(From a Letter to the Astronomer Royal.)

Of two unknown planets, found both on the night of July 31, and which may be called (122) and (123) provisionally, I have succeeded in making the following observations (omitting a rough estimation of position of (123) on the first night) :—

1. *Planet* (122).

1872.	H. C. M. T. h m s	App. α h m s	App. δ ° ′ ″
July 31	15 9 37	21 48 56·47	− 11 41 54·9
Aug. 1	11 32 49	48 22·36	45 1·8
2	12 42 0	47 39·73	48 57·4
3	12 1 16	46 59·52	52 38·2
4	12 1 18	46 17·64	11 56 31·0
5	11 57 8	45 35·47	12 0 21·1
7	12 7 10	44 9·32	8 13·1
8	11 9 40	21 43 28·11	− 12 12 4·4

2. *Planet* (123).

1872.	H. C. M. T. h m s	App. α h m s	App. δ ° ′ ″
Aug. 1	12 8 29	21 57 30·53	− 10 4 55·3
2	13 22 13	56 39·73	6 42·3
4	11 31 8	55 4·91	10 15·5
5	12 53 38	54 11·34	12 15·0
7	13 8 15	52 27·13	16 16·9
8	12 2 38	21 51 37·20	− 10 18 17·3

The magnitude of (122) I estimated 11·5, that of (123) a little less, perhaps 12·0.

Litchfield Observatory of Hamilton College,
Clinton, N. Y., Aug. 9, 1872.

Discovery of Minor Planet (124). By Prof. C. H. F. Peters.

(From a Letter to Sir G. B. Airy, Astronomer Royal).

Another new planet was found on 23 August, bright, of tenth magnitude. I communicate the following positions of the same, obtained by the filar micrometer:

1872.	Ham. Coll. M.T. h m s	α (124) h m s	δ (124) ° ′ ″	No. of Com.
August 23	12 28 7	22 21 23·61	− 7 18 28·8	10
„ 24	13 22 50	22 21 21·66	7 18 42·3	5
„ 25	12 7·44	22 19 44·45	− 7 30 13·4	10

Litchfield Observatory of Hamilton College,
Clinton, Oneida Co., N. Y., August 26, 1872.

Discovery of a Minor Planet (125). By M. Prosper-Henry, Assistant-Astronomer of the Paris Observatory.

The observed position was

1872, Sept. 11, $15^h\ 47^m\ 35^s$, Paris M.T. R.A. $23^h\ 59^m\ 33^s$. S. Dec. $0^\circ\ 55'\ 57''$
Hourly motion in R.A. = $-\ 1^s\cdot9$ (deduced from half-an-hour's observation).
Hourly motion in D = $-\ 20''$.

Magnitude about 11·7.

The comparison star was 4612 of Argelander's zones.
The number of this planet will be provisionally (125).

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LIST OF PRESENTS

RECEIVED DURING THE SESSION OF 1871–72,

AND OF

BOOKS PURCHASED WITH THE TURNOR FUND

DURING THE SAME PERIOD,

FORMING

APPENDIX XXIII.

To the Catalogue of the Library of the Royal Astronomical Society.

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